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TESTS OF TEMPERAMENT AND PERSONALITY

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1. INTRODUCTION: TEMPERAMENT AND PERSONALITY.

THE aim of a test of temperament or personality should be to predict, on the basis of a subject's reactions to a simple, well-defined test situation how his reactions in real life are likely to be affected by his temperamental qualities and personality traits. By 'temperament' I mean, roughly speaking, the total organization of an individual's innate tendencies of the affective and conative type, as distinguished from innate sensory and mental abilities that are more specifically cognitive and from definitely motor capacities[1]; if one accepts a McDougallian theory of action it includes also individual differences in instinctive equipment. Possibly these tendencies are determined by bodily characteristics such as physique, endocrine activity, etc., though that is hardly a psychological problem. In the adult, at any rate, temperament cannot be observed pure; 'personality' is to be regarded as the functional resultant of temperamental qualities, of intelligence and other characteristics of the organism as influenced and overlaid by environment and training. The term 'character' has, it seems to me, a non-psychological meaning that is too firmly established, namely an assertive or determined personality, to be adopted in psychological discussion. At present one can scarcely be more specific with one's definitions; I believe that the most satisfactory solution as to their nature, if not the only solution, is to be obtained by individual and comparative psychological research along the lines of the direct observational method of approach described below. However, we can realize, to some extent, their extreme importance; for we can measure, theoretically at least, a subject's mental abilities and manual aptitudes for various skilled tasks, but a knowledge of these is insufficient for prophesying his success and efficiency in our present social, educational and industrial systems since it omits the affective and irrational influences of inherited and acquired emotional dispositions such as carefulness, persistence, 'push,' timidity, emotional instability, also sentiments and interests which essentially determine what he will make of his talents. Thus Bartlett[1] defines temperament as "a group of predisposing tendencies and feelings which may be used to characterize one person in contrast to another, and which seems to come to expression independently of special training...." Temperamental qualities "determine a man's attitude and responses towards any number of situations rather than to a single specific situation..., they begin to find expression extraordinarily early in individual mental life, and they appear to be capable of singular persistence from generation to generation."

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[1] Cf. C. Burt, *The Measurement of Mental Capacities*, 1927, Henderson Trust Lectures, No. VII, Edinburgh.

In actual life, when we wish to gauge the temperament or judge the personality, say, of a schoolchild or a candidate for a job, we generally use the ordinary personal interview; but this is far too subjective and unsystematic to be trustworthy. However broadminded and dispassionate we attempt to be we cannot avoid the 'halo' effect, hence the technique of the rating method has been elaborated in an attempt to cancel out our personal prejudices and similar attendant disadvantages. It can seldom be said to have succeeded practically owing to the difficulty of getting suitable raters; in fact one must admit that the method often throws more light on the personality of the rater than the ratee; it is also too cumbersome to be serviceable outside the psychological laboratory. Hence there have developed, chiefly in America, innumerable tests of temperament and personality which claim to be scientific and objective, not depending on the vagaries of personal judgments. From the psychologist's point of view, however, the criterion for a good test must, ultimately, be the extent to which its results agree with those of trustworthy ratings.

I intend in this paper to consider one of the best known of these tests, to indicate some of its drawbacks and then to describe another method which is a compromise between the unsatisfactory extremes of the objective test and the subjective interview.

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[1] F. C. Bartlett, "Temperament and Social Class," *Eugenics Rev.* 1928, xx (1), 25–28.

2. THE DOWNEY WILL-TEMPERAMENT TEST.

The June Downey Will-Temperament Test, it is asserted, gives fairly accurate and objective measures of twelve traits. These are deduced chiefly from the subject's handwriting, since Downey[1] tries to show that temperament is expressed not so much in its shape and details (the graphologists fallacy) as in its speed, modifiability and the like. This theory also finds support from Galton[2] who states that the handwriting of identical twins is often the only measurable difference between them, so that "it would appear that the handwriting is a very delicate test of difference in organization." The details of the series of separate tests and the traits that they are meant to measure are familiar by now and need not be described again here; they have been summarized recently by Collins[3] and Oates[4] in this *Journal*. The traits fall into three groups, firstly those indicating speed and fluidity of reaction, secondly aggressive qualities, thirdly carefulness and persistence. They are marked out of 10 on the basis of empirical norms and are then plotted on a graph called the will profile whose shape is said to be highly characteristic of each subject's reactions in ordinary life. The total score for all the traits should indicate general strength or weakness of personality.

The test has been thoroughly tried out and there have been one or two moderately successful attempts to explain disparities between scholastic success and intelligence scores by means of it; also it is said that independent judges can sometimes recognize whom the will profiles represent. But on the whole it is most unsatisfactory for reasons that I hope to demonstrate, and most investigators only allow that it deserves notice as a pioneer effort, and that, on the analogy of early tests of intelligence, it is likely to lead to better results in the course of time.

Some of the main criticims are as follows:

(*a*) A general drawback to the whole series of tests, that can hardly be avoided, is that Downey's directions and norms for scoring are quite unsuitable for English adults; my subjects were of a fairly homogeneous type, hence their variation in performance was small, and their average mark, especially in the tests for Speed of Movement, Flexibility and Co-ordination of Impulses, was well above Downey's published average, namely about 7 marks out of 10 instead of 5[1]. This means that every investigator has to work out his own arbitrary norms in order to obtain adequate comparative measures of his subjects' temperamental traits. There are also several difficulties in the details of the application of the tests which each investigator has to solve for himself; some of these are given by Collins[2], but there is no need to recapitulate them since there are many other more fundamental objections.

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[1] J. Downey, *The Will-Temperament and its Testing*, New York, 1923.

[2] F. Galton, *Inquiries into the Human Faculty*, 1883, p. 157 in 1907 ed.

[3] M. Collins, "Character and Temperament Tests," this *Journal*, 1925, XVI, 89–99.

[4] D. W. Oates, "An Experimental Study of Temperament," this *Journal*, 1928, XIX, 1–30.

(*b*) The tests are unreliable; Rugg[3] gives 0·4 as the probable average correlation between two applications of the test to the same subjects. We scarcely realize the significance of such a coefficient; in another set of experiments I obtained this figure between two sets of scores for 150 subjects, so that it was nearly 20 times its P.E., and yet 2 per cent. of the subjects showed differences of 85 per cent. in their two scores, 10 per cent. differed more than 60 per cent., 20 per cent. differed more than 40 per cent. and over a half differed 25 per cent. or more. In other words, taking a physical analogy, this is as if one measured the boiling point of water one day and found it to be 100°, and on the next day there was an even chance of its being 68° or 132°.

In a single application of the tests for Freedom from Load and Motor Inhibition several trials are given in which I noticed highly divergent results that cannot be accounted for as practice effects; in the former there was an average difference between the first and second sets of trials corresponding to 2 marks, one way or the other.

(*c*) The correlation with ratings, as worked out by other investigators is very low: May[4] gives an average of 0·1 to 0·24 according to the methods used. The ratings themselves are, however, quite unreliable, so that this only proves that the raters do not know what the traits are supposed to mean. Hence the tests are of little or no practical or diagnostic value, but are still merely a psychological curiosity. I tested 23 subjects myself, with whose temperament and personality in ordinary life I am familiar; from consideration of their performances there often appears to be some correspondence with their characteristic qualities and traits, yet there is almost as much gross disagreement. This fault I hold to be inherent in the whole conception of the objective test of temperament, one that is unlikely to be eradicated however much further work is done on tests of this type.

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[1] D. W. Oates (*op. cit.*) also raises this criticism, though, as regards these particular traits, his schoolboys, tested by the group form of the test, were below, not above Downey's average.

[2] M. Collins, *op. cit.*

[3] H. O. Rugg, "The Downey Will-Temperament Test," *Journ. App. Psych.* 1923, VII, 65–76.

[4] M. A. May, "The Present Status of Will-Temperament Testing," *Journ. App. Psych.* 1925, IX, 29–52.

(*d*) Many of the tests seem to depend on motor capacities and habits, not psychological traits. Speed of Movement and Freedom from Load are almost certainly improved by experience in writing essays or taking lecture notes. Flexibility and Interest in Detail are affected by practice in drawing and copying. Motor Inhibition is helped by practice with microscopic work, dissection, drawing in detail, also by the possession of a large handwriting. A high score on Co-ordination of Impulses must to some extent depend on the natural speed and size of writing, though Downey denies this; I find that it certainly goes with a capacity to play violins or other stringed instruments, and so probably depends on practice, though of course Downey might assert, conversely, that this temperamental quality helps in playing a stringed instrument. Volitional Perseveration probably depends on habitual slowness[1] and lack of intelligence. If this muscular bias be admitted, some of the tests do seem to measure something besides themselves, especially Speed of Movement, the second part of Flexibility (the capacity for rapid imitation of an unfamiliar handwriting) and Motor Impulsion.

(*e*) Several of the tests depend largely on subjective judgments for their scorings; thus it is already acknowledged by the deviser that all the traits that constitute her conception of will temperament cannot be measured objectively. These more definitely psychological estimates seem to agree even less with real life and with ratings. The fundamental reason for this is that scarcely any account is taken of the mental state of the subject, the meaning the situations have for him. Speed of Decision and Finality of Judgment, for example, set up an intensely complicated attitude in the subject. He is asked to rate himself on a number of character traits, or to underline one of a pair of traits that best describes himself; apart from the obvious differences that will arise according to which alternative he adopts, the results will depend on his self-consciousness, his inhibitions, his lack of conscientiousness (which can hardly be identified with Downey's "speed of decision or lack of rambling and excessive caution"), the amount of self-analysis to which he is used, and, especially, his personal relations with the investigator. In the tests for Flexibility and Volitional Perseveration that require the disguising of his handwriting, the determining subjective factors are equally incalculable. The tests for Reaction to Contradiction and Resistance to

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[1] M. Collins (*op. cit.*) comes to the same conclusion as regards this test.

Opposition are particularly unsatisfactory, and depend also on the attitude of the subject to the investigator. In the former the subject chooses one of two envelopes and is later contradicted as to his choice; none of my subjects acknowledged that they had forgotten their choice (as they should have if they had been very unaggressive), but also, quite naturally, none of them troubled to maintain their opinions (as they should have if they had been very aggressive), the situation being an intrinsically futile one. In the latter, where they write with their eyes closed, and are supposed to react aggressively or submissively to an obstacle placed in the way, they usually thought that their pencils were moving off the paper and that I was guiding them; hence, by the use of more appropriate directions, the subjective situation had to be carefully prepared beforehand. The test for Motor Inhibition (including its four practice trials) would often take an hour or more to perform and may therefore be affected by such irrelevant factors as the subject's desire not to be late for lunch[1].

Half the traits depend on the use of tricks in the tests (Speed of Decision, Contradiction, Opposition, Finality, Interest and Perseveration), such as timing the subject when he has been told that he has unlimited time; I cannot believe that this practice is valid; for one thing, few stopwatches are silent, and innumerable, indefinable subjective factors invariably enter and distort the reactions.

It may well be that the fact that my subjects formed, on the whole, an intellectual, and therefore somewhat selected group, is responsible for the poor results I obtained. Working with children other investigators often seem to be more satisfied than I am. But surely it is hardly psychologically sound for a test to rely on the credulity and lack of sophistication of children and, apparently, the average American experimental subject.

3. FUNDAMENTAL DRAWBACKS TO THE OBJECTIVE TEST: NECESSITY FOR A QUALITATIVE METHOD OF DIRECT OBSERVATION.

I maintain that the whole objective method rests on a fallacy that the *Gestalt* psychologists call the Constancy Hypothesis, namely that identical, standardized objective situations or external stimuli are equivalent to similar standardized subjective ones, both for one person at different times and for different people at the same time. Psychological testers seem to think that definite and invariable test directions and conditions should be aimed at when it is clear that these will produce entirely different subjective situations in different people. In fact, by trying to be scientific, their methods are essentially unscientific; they are acting as a physicist who might try to prove Joule's law using different apparatus at every attempt, a mercury thermometer one day, a hydrogen one the next, and so on, without considering any of the vagaries of these instruments. Orthodox scientific method cannot take into account the variability and the sources of inaccuracy in psychological material, namely the human mind in all its complexity. The basis of more enlightened scientific method is systematization of the phenomena with which it deals, not the neglect of the factors it cannot easily control. It is gradually being realized that this type of experimental psychology forgets the individual and that a more clinical type of approach is needed. Even an ideal set of tests of temperament and personality which attained perfect reliability and which agreed exactly with external ratings would only establish an artificial statistical individual, as Viteles[1] puts it, a mosaic of "isolated sectors of the individual ego." Similarly, Allport[2] recognizes that personality traits merely represent groups of characteristic reactions of the individual selected and abstracted for consideration; personality or temperament is a co-ordinated whole, an integration or configuration of all these abstractions that cannot be put into objective terms or measured satisfactorily.

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[1] This difficulty can be overcome by substituting the group form of the test in which, instead of taking as long as possible to write 'United States of America' (up to 30 minutes), the subject traces a scroll with his pencil, doing as little as possible in a definite time (2½ minutes with ½ and 1½ minutes practice trials).

Temperament is, in fact, qualitative rather than quantitative; we ought to consider the way a subject reacts in a number of situations rather than how much he reacts in a specific situation, as shown by Bartlett's definition, given above. Burt[3] and other educational psychologists have elaborated a method whereby they simply observe children's modes of reaction while answering specifically devised questions and in situations such as Binet testing, or even during expeditions to the shops, the cinema or the Zoo. From these objective observations they deduce the children's temperaments subjectively, intuitively one might almost say, and by this empirical method they are able to obtain results that are reliable and of immediate and practical value. Even the Downey tests can be used to give several good qualitative indications of this type; for example, in Speed of Decision, using a rather less embarrassing set of character traits, I gave up trying to time the test secretly but observed each subject's carefulness, frankness, tendency to quibble, etc., and also took some account of his actual self-estimates, judging whether he seemed fair or biassed. Even more significant was the way in which he did the Motor Inhibition test, his self-control, unsteadiness and remarks. It was also possible to supplement these indications by noting the amounts of scroll covered in successive half minutes; those who found the test the greatest strain and who, from other observations, tended to be generally temperamentally unstable, often varied as much as 30 per cent. in their speed. It should also be possible to devise good tests for Resistance to Opposition and Interest in Detail in which there is no trick but in which the subjects' normal reactions could be qualitatively observed.

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[1] M. S. Viteles, "The Clinical Viewpoint in Accident Prevention, Symposium on Accidents," *Proceedings of the Vth International Psychotechnical Conference, Utrecht,* 1928.

[2] G. W. Allport, "The Study of the Undivided Personality," *Journ. Abn. Soc. Psych.* 1924, XIX, 132–41.

[3] C. Burt, *The Young Delinquent,* London Univ. Press, 1925.

I seem to be advocating a regression from the attempted scientific methods of the supposedly objective test to the subjective inaccuracies of the personal interview. The difference from the ordinary interview, however, is that this approach is, as far as possible, systematized and controlled, for the test performances to be observed provide definite objective standards on the basis of which different subjects may be qualitatively compared; specific types of behaviour emerge during these performances which can be recorded objectively and so serve to minimize the investigator's or interviewer's subjective prejudices. The actual performances chosen do not matter very much as long as they give plenty of opportunity for the subject to show himself up; they must be such that the whole personality takes part in doing them, and so becomes mirrored in the resulting behaviour and can then be recorded. Hence a simple motor test is not likely to be of much use in that it may often be performed as an automatic and specialized habit. Naturally the ideal would be for the interviewer to observe closely each subject's reactions in ordinary life over a long period, but if a number of different tasks are provided the whole process can be considerably shortened, the method will be much more practical and need not become artificial, and at the same time may be more objective and unbiassed than such protracted observation might tend to be.

4. APPLICATION OF THIS METHOD WITH PERFORMANCE TESTS.

As a tentative exploration of this method I collected and gave to 12 of my Will-Temperament Test subjects a number of performance tests and games, of a simple subjective nature, namely:

1. "Anchor" BRICKS (No. 13, "The Tormenter")[1]. A number of small, angular bricks of uniform height, but different shapes, are to be built into a variety of two-dimensional patterns, squares, polygons, etc., all degrees of difficulty being available. They take from about 1 to 25 minutes to accomplish.

2. GUIDIT. A small steel ball is chased up an inclined plane to a goal at the top with a knitting needle; in the plane are several holes past which the ball has to be guided, there are also various diagonal barriers round which it must be taken, so that progress becomes more and more difficult as the top is approached. If the ball falls through a hole or off the edge it must be started again from the bottom.

3. A LOCK and door handle mechanism has to be assembled from about nine component parts. Those who are unfamiliar with locks take about 20 minutes over it.

4. DESIGNS. A number of pieces of card, triangles, squares, etc., of ten different colours are provided. The subject chooses whichever of these he likes and arranges them on a white ground into any pattern, design or picture which he regards as the best he can produce with the available material.

5. CUBES. Fifteen cubes of wood (sides $\frac{1}{2}$ in.) have to be built up in a pile on top of one another. The sides are badly cut, neither being at true right angles with each other, nor even strictly plane, hence this is practically an impossible task.

6. An ordinary postcard size JIGSAW Puzzle, representing a landscape.

7. The Downey SCROLL test for Motor Inhibition, as described above in the note on p. 102.

These tests were not given in a laboratory, nor under strict experimental conditions. Either I showed them to a subject in order to see if any spontaneous interest was evoked, or else asked him to "play some games" for me, for psychological purposes. I usually explained that I was not interested in objective measurements of his quickness (thus a first success at GUIDIT occupies from 5 to 40 minutes, with practice 2 minutes is a fair average, so that the actual time taken has very little temperamental significance), but said that I wished him to do them quite naturally so that I could see in what way he did them, whether he liked them or not, and found them easy and interesting or difficult and irritating, and so on. I then simply observed whether any obvious qualitative individual differences existed in the performances of various subjects, and, in spite of the many difficulties of this method, a number of temperamental traits showed up immediately, ones which I know and which, other people agreed, the subjects exhibit in ordinary life.

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[1] I should like to take this opportunity of expressing my thanks to Miss Fildes, who gave me several useful suggestions and told me of this test and of the Cubes (No. 5), which she has previously applied to children with similar results.

Each subject must come at least twice; half-a-dozen times would be desirable in order that he may be given as many opportunities as possible of displaying his temperament from different aspects. While he attempted the test I wrote down as many notes as to his behaviour, mood, etc., as I could, keeping separately the actual facts of observation and my immediate and subsequent deductions. It is essential for the investigator to try to eliminate any previous opinions about his subject's personality. His mind must be as alert and fresh as possible, he should try to enter into each subject's thoughts and feelings, to understand, simplify and control the subjective situation. Allport[1] shows that great individual differences exist between different investigators in their intuitive capacity, and suggests that the personalities of their subjects must be apprehended empathically.

Hence each subject should be treated differently; to one the whole nature of the test may be explained, this would put another on his guard, and he might, for example, repress his habitual emotionality, therefore he must not be enlightened quite so much. A third should be stimulated to concentrate on the tests, a fourth should be allowed to smoke and talk freely, and so on. The question of giving hints when the test goes on too long, or suggesting that subjects should or should not give it up is also a very variable and subjective one. Undoubtedly far more standardization of instructions, method, etc., is desirable, without their becoming stereotyped and artificial; at present the procedure is so indefinite that too much depends on the good faith of the investigator, and how far he can overcome his halo effects. There is also a considerable danger of drawing conclusions about a subject's temperament from one test, and only taking full note of reactions to subsequent tests that fit in with these conclusions.

But there is certainly a very pleasing degree of objectivity in the test, namely that so many actual remarks and reactions can be recorded, and reconsidered subsequently till subjective biases are overcome; introspection on the part of the subject is unnecessary (an important point to remember in view of a possible application of the method to education or industry), and, when it was asked for, it only served to confirm the objective observations already obtained. The method is, in fact, closely similar to that of Köhler[1] who set his apes several problems and watched to see how they would solve them, his deductions being based not on any actual measurements, but simply on the behaviour of the animals and his intuitions as to their feelings. I myself was frequently struck by the resemblance of my subjects to the apes who, when thwarted by some obstacle from obtaining food, expressed rage or despair or became thoroughly bored with the situation until, suddenly, the true solution dawned on them and was carried out rapidly and completely. The apes also showed similar temperamental differences in their reactions to all the test situations, hence Köhler is able to describe their personalities.

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[1] G. W. Allport, *op. cit.*

A further obvious drawback to these tests is that they are games which will be reacted to differently from situations in real life. Actually it appeared that it was no use trying to perform them in this dissociated way; in constructing the lock, for example, one's mind soon becomes absorbed (that is, if one's mind is temperamentally ever capable of concentrating on anything), every possible item of experience or method of solution that might help is resuscitated. So that though the objection might apply to one or two of the tests, yet when a subject can be proved to show the same traits in several situations on different occasions, it seems fairly justifiable to assume that these same traits are likely to appear in his reactions in ordinary life.

5. RESULTS OBTAINED.

My subjects were too few in number to make it worth while determining statistical correlations between external ratings and the results of tests; moreover, it is most difficult to put these results into adequate quantitative terms. Hence, in the present investigation, I had to rely on my own observations in ordinary life and discussions of the personalities of my subjects with other people as standards against which their behaviour in test situations could be compared. I will therefore abstract certain traits that seemed to emerge most obviously and describe shortly the reactions of those who exhibited their presence or absence in the most extreme form.

Every test gives some indication of a general but important trait, Constructive Interest, creativeness or inventiveness; perhaps with McDougall we should call it an instinct. To be able to test for it would be most valuable, for those who are deficient in it would be best adapted for the monotonous work so common in modern industry, those who are superior in it can never be satisfied with such automatic tasks. It may be remembered that Köhler's apes often showed definite aesthetic tendencies in the form of aimless, playful reactions, adorning themselves with rags, branches and the like. Nueva even managed to construct a primitive knot without any utilitarian purpose, and Sultan, when he had succeeded in making a double stick with which he could reach the fruit outside his cage did not stop to eat it but pulled in all sorts of useless objects.

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[1] W. Köhler, *The Mentality of Apes*, 1925, translated from 2nd edition by E. Winter.

Subject A. is typically of what is called the artistic temperament in ordinary life. In doing GUIDIT, although he showed great emotional instability (see below), he was spontaneously 'fascinated by it.' He chose the other tests more or less in order of constructiveness, namely DESIGNS, JIGSAW, LOCK, BRICKS, CUBES, saying, for example, that he preferred LOCK to BRICKS "because it has more variety."

At DESIGNS he lost all sense of time and went on spontaneously for two and a half hours at one sitting; during this time he completed about 20 designs, fully exploiting the most striking combinations that the other subjects hardly attempted. He continually said, "I do love this game," again, "I like this game, there are no mechanical principles." His enthusiasm prevented me from becoming at all bored at merely watching him, and though, naturally, any investigator will have particular preferences, I certainly appreciated his colour combinations more than those of any other subject. He appeared to be more interested in colour schemes than in form, the latter seeming to be implicit and effective without needing conscious consideration; he judged the colours that went together well subjectively (in Bullough, Valentine and Myers' sense). He showed distinct empathic tendencies, *e.g.* arranging darker colours near the bottom and on the side farthest from the window, or else, by combinations of light and shade, obtaining remarkable three-dimensional effects. His symmetries were always vertical and diagonal rather than horizontal, thus if AB, CD, etc., represent various patches of colour or groups of coloured pieces, he used schemes such as $\begin{matrix}AB & BA\\ CD & DC\end{matrix}$ or $\begin{matrix}AB & AB\\ DC & DC\end{matrix}$ rather than the more conventional $\begin{matrix}AB & & BA\\ & C & \\ AB & & BA\end{matrix}$. He had no wish to make representational designs or pictures and deprecated the suggestion.

The JIGSAW he built up characteristically with reference to the whole picture, synthetically rather than analytically. In doing the BRICKS he frequently tended to design his own patterns, forgetting the set ones. Two methods of balance are possible in the CUBES, either the angles may be carefully examined and complementary ones fitted together so as to produce a straight pile, or else successive cubes may be made to overhang the ones below them in order to compensate for badness of fit, producing what one might call a ‘crazy chimney’ effect. A. adopted the latter and took pleasure in the result as such.

At the opposite extreme subject D., who is characterized by somewhat prejudiced and unimaginative artistic tastes, was only interested in the tests when immediately successful at them (see below, Persistence), and showed none of these aesthetic tendencies. Similarly, B., E., I. and K., though not lacking in artistic taste, are seldom if ever aesthetically creative in real life; they were incapable of producing satisfactory abstract designs and tended, instead, to make representational ones, pictures of buildings, boats and the like. Subject I. said, “I cannot imagine a design being pleasing” and proceeded to amuse himself and me with trees, birds and houses.

Contrasted with the aesthetic tendencies are the mechanical or scientific trends of several personalities; thus the LOCK appealed most to E. and K.; the former, though he was quite unfamiliar with this type of mechanism, had put all the pieces together except the spring in 4½ minutes, and after carefully working out the principles completed it in about 9 minutes, half the time taken by any other subject. Similarly, by means of close consideration of the angles of the CUBES he obtained a pile 13 cubes high at his first attempt, and his average height before falling was 12, the more normal figure being 8 to 9. The BRICKS constitute the best single test since there is opportunity for exhibition of both aesthetic and scientific tendencies. E. was again the subject who made most use of analytical and geometrical principles, always effectively working out the proportions of the various pieces instead of using overt trail and error methods, or, as most of the subjects, depending on the attainment of correct configurations by chance.

We have seen already how artificial are the abstractions we call temperamental traits; each one that I consider overlaps, positively or negatively with every other. Hence scientific Constructiveness is not very different from what might be called Planning Capacity. This is not the same as intelligence, at least agreement with marks obtained on specially devised intelligence tests was nil; but the agreement with ratings of the same subjects on intelligence was fair, so that some kind of intellectual power rather than alertness and brightness is indicated by these performance tests. Subjects G. and H. demonstrated very clearly the lack of it; the former started off on GUIDIT with a very inflexible method of balancing; she quite failed to realize that once the ball starts to run down the needle, say to the right, so that the right-hand end is raised to stop it, that directly after the left-hand end must be raised to prevent the ball running back in the opposite direction. Hence her failures differed from those of the better balancers in that the ball fell off the edge of the plane much more frequently than into the holes; usually the reverse holds. Again she made no attempt to take account of the irregularities of the surface of the plane at different places. She constructed the JIGSAW more or less blindly, taking but little account of the whole, and was very slow at realizing when pieces did not fit into each other, often forcing the wrong ones together. In the BRICKS she continually made the same mistakes; though often getting the pattern almost correct, by chance, she failed to see the logical solution. One of these patterns was a square, and on arriving, fortuitously, at a rectangle she thought she had succeeded and took several seconds to realize the mistake. She always built up the patterns in small pieces, with no relation to the whole. Yet she differed from H. in being interested and alert throughout.

H. yawned frequently and sighed; she similarly exhibited poor balancing reactions in GUIDIT; in the LOCK she never seemed to think out the functions of the parts but tried the pieces, blindly, with listless movements and without any active interest. G. had remarked that she preferred BRICK to LOCK since, "I should never be able to do it, even if I saw you take it to pieces," while H. soon gave up saying, "My mechanical brain is not very good." The latter was equally lacking in any 'geometric sense,' for example she was unable to see that a certain right-angled triangle was equal in area to a certain square, so that by slight readjustment she could have at once completed one of the patterns. Instead she pulled the pattern to pieces, and, by doing it by trial and error, failed to get it right. It may be noted that they both scored very low on Downey's Interest in Detail test, though the agreement between this and Planning Capacity was not very close.

They may be contrasted with K., who was mentioned above, or with C. who looked at the pieces often for 1½ minutes at a time, thinking out their proportions, etc., rather than using trial and error methods. Similarly, he considered the coloured pieces for 3 minutes before he began making a DESIGN. He carefully analysed the irregularities of the surface of the plane in GUIDIT, remarking, "I must plan out the best way first"; his movements were slow and controlled, and he rolled up his coat sleeves in order to attack it better. His method of building the CUBES and his successes were similar to those of K. This trait also seemed to be related to the subject's degree of concentration on and attention to the tasks; some tended to get bored and talk about irrelevant affairs, others showed an active, fighting attitude. The latter, when held up, often tapped the table or floor with their fingers or foot, or whistled softly.

Closely connected also, though perhaps more in the nature of muscular abilities, were the manual control and steadiness in CUBES and GUIDIT.

Though they are not wholly mutually incompatible, a trait that one may call Emotional Instability is, to some extent, the reverse of Planning Capacity. This bore a fairly close relation to one of Downey's best tests, that for Motor Impulsion or impetuosity. But emotional traits are very easily obscured by the suppression that the subject manages to maintain, and hence on his attitude towards the investigator; presumably more searching and prolonged tests of a suitable kind would eventually break it down, if it is ever broken down in ordinary life.

Subject A., to whose 'artistic temperament' I have already referred, exhibited the trait to a high degree. In the Downey SCROLL he worked steadily for short periods at 2 units per half minute, but when engaged on it for 2½ minutes he continuously quickened up, reaching 4 units in the last half minute, saying at the end that it "made me feel all quivery." His score for Motor Impulsion was 8.

In GUIDIT he averaged at least 4 to 5 falls a minute; he often knocked the ball right over the top of the plane on to the floor several feet away by rushing it up too quickly or pressing it violently against the barriers. At almost every fall he cursed or thumped the table; he made frequent remarks about his unsteady hand and showed every sign of exasperation. I neither interfered nor stimulated him in any way and, with occasional periods of self-control and steadiness, he spent 40 minutes in obtaining his first success. While doing the BRICKS he alternated between emotionality and seriousness, sometimes breaking into hysterical laughter when the pattern turned out to be wrong. He pulled these unsuccessful attempts to pieces childishly and started again from the beginning. One or two of his remarks were, "It is irritating that the bricks are not all of the same colour and surface," and "This is a short cut to madness."

He showed considerable variability on different days, but though comparatively quiet while doing the cubes the average height of his piles was only 7. In the more constructive tasks, LOCK, DESIGNS, JIGSAW and in making his own patterns with the BRICKS he was far calmer.

Subject I., on the contrary, was absolutely steady with the SCROLL, and drew it more slowly than any other subject, namely at 0·225 units per half minute. In GUIDIT he only averaged 1½ falls a minute; his movements were slow and controlled and he reached the top in 14½ minutes. He often whistled under his breath while doing the BRICKS, and though lacking in Planning Capacity, he attempted to work out the patterns geometrically, and did not show emotional or wilful movements. When given the CUBES he said, "I am going to do this in spite of you"; "You will not easily make me get annoyed, I do not normally do so"; he laughed at his falls but without exasperation and took great care in picking out the firmest ones for the base.

K.'s emotional stability has already been mentioned under the heading of scientific Constructive Interest. The nearest approach to a complaint or exasperation of any kind was a remark about the BRICKS, "This must be very ingenious," and "I am sorry I am taking such an awful lot of time over this."

Though it is not easy to give verbal illustrations, yet there certainly seemed to me to emerge two emotional types, on the one hand those who maintain an even level of moderate emotional reactions, and on the other hand those who show great variations and inconsistencies, alternately becoming very uncontrolled and then quiet again; these variations or lack of variations appear in different tests on different days or in the course of one test[1].

Some indications of the commonly accepted *extravert-introvert* distinction were obtained; subjects B., D., E., H., I. and K. would be acknowledged by any external judge to be predominantly extravert in everyday life. All of them, and none of the rest of my subjects, showed the characteristic of asking how their performances compared with those of other people, or else what I thought of their performances. B., for example, in GUIDIT said, "How far do people usually get with this?", "This must be boring for you"; in the CUBES, "What is the record with this?"; H. and K., similarly, with GUIDIT asked, "Does anyone ever get to the top?" and "Don't people reach the top earlier than this?" Another characteristic, shown especially by E., H. and K. was a marked *suggestibility* in GUIDIT. About one-quarter and half the way up are two places that are especially difficult to pass; some time after starting the test these subjects tended to get stuck at these places, even if they had passed them several times before; H., for instance, spent 5 minutes, failing at the lower hole and 4 minutes at the upper, while once she had broken the spell and regained her self-confidence she had hardly any difficulty with them. It is clear that the influence of competition and other temperamental factors connected with social and gregarious qualities could be demonstrated by further work on tests of this type. Thus F. showed continual tendencies to childish emotional reactions and exasperation, but when a third person entered the room (though paying no attention to the experiments) he controlled himself far more, while the opposite effect was observed once with subject A.

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[1] This same distinction is noted by M. D. Vernon, "On Certain Effects of Long Spells of Repetitive Work," this *Journal*, 1926, XVI, 222–36. One of her subjects showed the same emotional variability characteristics in her tests and mine.

Finally there is the important trait of Persistence, also closely bound up with Planning Capacity; the actual times spent on the tests before giving up seems to me to depend on a complex of factors such as (*a*) actual personal interest in the task; some may be more persistent at the BRICKS than at the less constructive GUIDIT, though in general the same degree of Persistence did seem to be exhibited in all the tasks; (*b*) self-assertion or vanity that makes subjects object to being beaten by a simple test; usually (*a*) starts at a high level, dwindles and (*b*) keeps them going; they alternate between satisfaction, hope and despair. A few develop more of an aesthetic or Constructive interest as they go on. (*c*) Indirect interest, *i.e.* the extent to which the subject thinks that I expect him to go on, how well his performance stands with reference to those of others, etc. The investigator should of course do his best to standardize this, so that it may exert the same subjective influence on all the subjects. At the extremes, however, I think that it played very little part. Take, for instance, D., who notably lacks Persistence and determination in ordinary life. At first he was highly self-confident and was delighted with GUIDIT; but after only four falls, at 3 minutes, he said, "I am not very successful at this sort of thing." A minute later he nearly reached the top and said, "I like this game" and "This is rather a good game." But 3 minutes later he gave up spontaneously with, "Here, I think I'd better try your lock"; he had only had 9 falls altogether, as contrasted with A.'s 150 to 200 falls. He added that he would find the lock easy as he was interested in physics. But in 1½ minutes, "I have no idea how these things go. This beats me"—2 minutes, "I have never seen anything like this before." He fumbled hopelessly, not even trying to work out how the key fitted, although quite intelligent. At 6 minutes, "This means absolutely nothing to me," then he hedged and rationalized, "Now if you had given me wires to solder, I am good at that." At my suggestion he was very glad to give up and try the BRICKS, and was extremely pleased at completing an easy pattern in 1½ minutes. When given a harder one he soon tended to talk about irrelevant matters. At

3½ minutes, "There do not seem to be enough pieces to make this one" and murmured (not for me to hear), "Impossible." He gave up at 5¼ minutes.

E. and K., on the other hand, only asked whether I did not get bored at watching them for such a long time. E. spent 8 minutes fitting in the final piece of the LOCK, without showing any signs of giving up. When given a very difficult BRICKS pattern he remarked at 18 minutes, "So far no solution, but let's have another try," and took 25 minutes in all. When asked he replied, "I would have gone on much longer but might have been rather bored." K. at the LOCK apologized for taking such a long time, and at 17 minutes said, "Do you mind if I go on and finish this?" He succeeded in 23½ minutes without any tendency to give up. He built up piles of cubes for 21 minutes till I stopped him. After taking 22 minutes for a first success at GUIDIT he asked if he was to continue.

Such big differences as this can hardly be put down to subjective influences exerted by the investigator. It may be noted in this connection that psychologists make very bad subjects for the tests; for example, subject I. remarked while doing the BRICKS, "I must do the normal thing," "Is it persistence you are getting at?", "You are wondering if I am going to give up sooner than the others." Again, at the CUBES, "It is no good your trying to measure my persistence, as I can go on at this any length of time." Their experimental bias makes it impossible for them to approach most of the tests naturally, though DESIGNS was fairly free from these artificial factors.

6. Conclusion.

We see, then, that it is possible, with the aid of these tests, to obtain a very complete picture of each subject's temperament, and that the method is capable of considerable development and practical application. Thus I could guarantee, with BRICKS, LOCK and DESIGNS, to diagnose in about an hour whether aesthetic or scientific tendencies are predominant in any subject, without any introspection on his part or previous knowledge of him. There was a close agreement between the aesthetic qualities demonstrated in the tests and the artistry, emotional attitude, etc., in musical appreciation. I also suggested in an earlier article[1] that a definite personality type existed among men who are successful at rowing; it was characterized by suggestibility, determination and persistence, extraversion and various social qualities enabling them to fit in well with other members of their boat and of the Boat Club. I now feel confident of being able to diagnose the essential traits with the aid of my tests and with parts of the Downey Will-Temperament Test, since the latter often indicates useful motor qualities, even if it throws scarcely any light on more psychological factors. E. and K. are fairly representative of the type and their characteristics have been shown above to conform excellently with these specifications; they are fairly suggestible and extraverted, emotionally stable and persistent; nor are they too aesthetically inclined or introspective; their motor control in GUIDIT and CUBES was good. In the Downey tests good rowing men should not obtain any very high or low scores[1]; but their Speed, Load, Impulsion and total scores should be rather above the average, and their Motor Inhibition steady. Nearly half my Will-Temperament test subjects were rowing men, their profiles showed distinct tendencies to similarity in that their variability on any one of the sounder tests was much lower than that of the other half of my subjects. I was not familiar with K.'s rowing characteristics, but I managed to deduce from all the tests that he was somewhat slow in the boat, but was very persistent, and not easily upset or hustled, also that, socially, he got on very well in any boat that he was in; these deductions were entirely confirmed by subsequent enquiry.

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[1] P. E. Vernon, *The Psychology of Rowing*, this *Journal*, 1928, XVIII, 317–31.

But these experiments were, of course, tentative and inadequate; more tasks and more subjects are required; tests could be arranged to give many more objective indications. The results depend at present too much on the investigator; his judgments should also be compared with the ratings of those who base their estimates on the characteristics of each subject in ordinary life. But I feel justified in maintaining that this approach is more satisfactory than any of the more objective methods, such as the Will-Temperament test, that have been devised so far, in that it attempts to take account of the subjective situation, is less artificial and is more likely to lead to results that may be used in the actual practical diagnosis of personality and temperament. Further tasks that might be used are, firstly, essays or discussions on set problems, and secondly, some kind of apparatus on which the subject might be given cognitive tasks in the performance of which he would exhibit his temperamental qualities; in neither of these need the real object of the experiment be mentioned and more natural behaviour might be expected.

In conclusion, I found unexpected support recently from a number of continental psychologists[2]; in many German and other laboratories temperament is determined by this direct observational or qualitative method applied in any of the psychological experiments that various subjects undergo[1]. For example, Nagy[2] maintains the following points: in the testing of temperament conditions should be as normal as possible; qualitative, not quantitative, tests should be used, relative not absolute principles should be applied; the subjective situation is the most important, and the investigator should use intuition as well as intellect, so that "Ein guter Psycholog muss nicht nur ein Gelehrter, sondern auch ein Künstler sein."

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[1] Cf. D. W. Oates' (*op. cit.*) conclusions with regard to success at school work.

[2] *E.g.* L. Fejgin-Garstensteyg, *Certaines Manifestations du Caractère et du Tempérament dans les Épreuves Psychotechniques*, and J. Suter, "Ueber die Ermittlung von Charaktereigenschaften bei der psychotechnischen Prüfung der Fähigkeiten," *Proceedings of the Vth International Psychotechnical Conference, Utrecht*, 1928.

SUMMARY.

(1) Temperament and personality are of extreme social, educational and industrial importance so that it is very necessary to devise satisfactory methods of diagnosing, estimating and measuring them, and of establishing more clearly their psychological nature.

(2) The rating method is scarcely practicable, although preferable to the ordinary personal interview.

(3) The following main criticisms may be raised against objective tests such as the June Downey Will-Temperament Test:

(*a*) The directions, details of application and norms are unsuitable for English adults and have to be worked out afresh by every investigator.

(*b*) The statistical reliability of the component tests is very low.

(*c*) The agreement with ratings is usually so poor as to make the test of little if any practical value.

(*d*) Many of the tests depend on motor capacities and habits rather than psychological traits.

(*e*) Others are not applied and scored really objectively.

(4) In general such tests neglect the complexity of the subjective factors involved; owing to their very objectivity and scientific orthodoxy they are artificial and psychologically unsound.

(5) Personality and temperament are organized and integrated wholes which cannot be put into simple quantitative terms; their estimation must be supplemented by direct qualitative observation and intutition.

(6) Another approach, intermediate between the objective test and the subjective interview, as used by Köhler, Burt and in several continental laboratories, was tried with far more promising results.

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[1] I believe that similar methods are used practically in the vocational guidance work of the National Institute of Industrial Psychology.

[2] L. Nagy, "Zweck und Methode der pädagogisch-psychologischen Prüfungen," *Proc. Vth Int. Psychotech. Conf.* 1928.

(7) Various simple performance tests and games were given to a number of subjects under conditions that were as normal as possible. Their remarks and behaviour were observed and many traits emerged.

(8) Chief of these were aesthetic and scientific constructive interest, planning capacity, emotional instability and variability, extraversion-introversion, suggestibility and persistence.

(9) They appear to agree well with the subjects' characteristics in real life, and the method is capable of considerable development, systematization and practical application.

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GROUP FACTORS IN TEMPERAMENT QUALITIES

By DAVID W. OATES.

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3. *Examination of the inter-correlations of the tests* (pp. 119–123).
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1. INTRODUCTION.

IN a previous paper[1] the present writer reported the results of an experimental study of temperament. The tests then used were critically examined, the temperament profiles obtained from 50 subjects were studied, and the possible relationship of temperament as tested by these tests to intelligence and scholastic success was examined in the light of scores obtained in the temperament tests by successful and unsuccessful students respectively—success being indicated by school achievement. The conclusions there reached were sufficient to suggest that the investigation should be continued to provide data for a more intensive analysis of the traits measured by the tests. The present paper is a direct continuation of the previous investigation; and its special object is to examine more fully the nature of the temperament qualities functioning in the tests, with a view to discovering the possible existence of one or more group factors.

2. SOURCES OF DATA.

(*a*) *Temperament tests*. The tests themselves were taken from the Downey Will-Temperament series, described in detail in the previous paper; the following were selected:

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[1] This *Journal*, July 1928, XIX, 1–30.

T 1. Speed of Movement.
T 2. Freedom from Load.
T 3. Flexibility.
T 4. Speed of Decision.
T 5. Motor Impulsion.
T 6. Self-Confidence.
T 7. Non-Compliance.
T 8. Finality of Judgment.
T 9. Motor Inhibition.
T 10. Interest in Detail.
T 11. Co-ordination of Impulses.
T 12. Volitional Perseveration.

(*b*) *Scholastic tests.* The subjects were students in a Secondary School. The marks obtained by them in two School Terminal Examinations, held at an interval of six months, provide the data for scholastic estimates. Other tests were administered during the period intervening between the two examinations.

(*c*) *Intelligence tests.* For estimating intelligence a battery consisting of the ten following tests was used: Obeying Orders; Mixed Sentences; Analogies; Number Series; Best Reason; Common-Sense; Cypher; Meaning of Words; Absurdities; Orientation.

(*d*) *Handicraft tests.* The medium of the Temperament Tests is handwriting executed under varying conditions. Burt[1] has shown that writing has a fairly high specific correlation with handwork and drawing; and has suggested that all three may involve a special ability or 'group factor,' doubtless consisting largely in an aptitude for skill and speed in finger movements, not unlike what is sometimes called motor ability. In order to test the possible relationship of the Temperament Tests to the motor factor present in handwork, a number of practical tests in woodwork consisting of marking out and making models in wood were carried out by the subjects of the investigation in the Handicraft Department, and the marks obtained for the exercises were used for calculating the correlations which appear below.

3. EXAMINATION OF THE INTER-CORRELATIONS OF THE TESTS.

The results summarized in Table I appear to indicate that there is little if any relationship between temperament and intelligence as measured by the tests used in this investigation. The coefficient of 0·111 becomes a partial coefficient of – 0·159 when the influence of school examinations is eliminated. This agrees with the results based upon the data reported in the previous paper[2] and with Stead's results[3] on an investigation in

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[1] C. Burt, *The Distribution and Relations of Educational Abilities*, pp. 58 and 61.

[2] This *Journal*, XIX (1), 17.

[3] H. G. Stead, "Factors in Mental and Scholastic Ability," this *Journal*, XVI, 3–207. Cf. A. A. Roback, *The Psychology of Character*, 1927, p. 156: "Neither intelligence nor volitional qualities enter into the temperamental make-up of a person; and on this point it is my impression that all are agreed."

Table I. *Inter-correlation of tests.*

	T 1	T 2	T 3	T 4	T 5	T 6	T 7	T 8	T 9	T 10	T 11	T 12	I	E 1	E 2
T 1	—	·614	·130	·117	·173	−·189	−·063	·187	−·027	·112	·308	−·020	·141	·019	·194
T 2	·614	—	−·083	·486	·158	−·010	·120	·296	−·100	·132	·322	−·046	·159	·349	·508
T 3	·130	−·083	—	·127	−·153	·010	−·197	−·034	−·183	·386	·049	·075	·078	·050	·219
T 4	·117	·486	·127	—	−·184	·168	·066	·227	−·081	·275	·435	·129	·071	·298	·488
T 5	·173	·158	−·153	−·184	—	·165	·043	−·034	·020	·084	·087	−·033	·025	−·018	·078
T 6	−·189	−·010	·010	·168	·165	—	·083	·205	−·142	−·037	−·003	−·350	·284	·253	·386
T 7	−·063	·120	−·197	·066	·043	·083	—	·032	−·110	−·039	·169	·150	·275	·303	·123
T 8	·187	·296	−·034	·227	−·034	·205	·032	—	·216	−·006	−·093	−·061	·174	·183	·294
T 9	−·027	−·100	−·183	−·081	·020	−·142	−·110	·216	—	·135	·047	·007	−·178	−·186	−·038
T 10	·112	·132	·386	·275	·084	·037	−·039	−·006	·135	—	·382	−·075	·247	·412	·445
T 11	·308	·322	·049	·435	·087	−·003	·169	−·093	·047	·382	—	−·045	·176	·401	·513
T 12	−·020	−·046	·075	·129	−·033	−·350	·150	−·061	·007	−·075	−·045	—	−·119	−·217	−·187
I	·141	·159	·078	·071	·025	·284	·275	·174	−·178	·247	·176	−·119	—	·589	·539
E 1	·201	·349	·050	·298	−·018	·253	·303	·183	−·186	·412	·401	−·217	·589	—	·811
E 2	·194	·508	·219	·488	·078	·386	·123	·294	−·038	·445	·513	−·187	·539	·811	—

which he used estimates for certain character traits where we have used the scores made in tests of temperament qualities. The T tests in Table I which have fairly high correlation with intelligence apparently involve some amount of intelligence in their performance. Test 6, in the form here used, as the author points out, "encourages an intellectual balancing of reasons rather than the immediate characteristic response[1]." The quality involved in Test 10 is measured by the exactness with which a model is copied, and Test 11, which correlates definitely with Test 10, also tests ability to act correctly as well as the ability to act quickly. This element of correctness involves the exercise of an intellectual factor. Test 7 again is related in type to Test 11 and correlates highly with it. Tests 1 and 2 probably correlate with intelligence on account of the speed element. Most of the intelligence tests are worked with definite time limits, and speedy workers are likely to do well in these tests and also in T tests 1 and 2. Such relationship between the temperament tests and intelligence as the figures in our table indicate, is probably to be attributed to the influence of certain temperamental qualities upon the performance of subjects in the intelligence test, rather than to the existence of an intelligence factor in the T tests themselves. We may assume, therefore, that intelligence does not enter as a general factor into the T tests as a whole, but that it operates to a slight extent in a few of the tests only.

Most of the T tests have a higher average correlation with the two school examinations than with the other T tests; for example, T 6 has a coefficient of 0·320 with the examination results and – 0·026 with the other T tests, and T 10 has coefficients of 0·429 and 0·129 respectively. The correlation between the school examinations and temperament tests, therefore, suggests the possibility of a factor common to both. If '*g*' is not a general factor in the T tests, as our results above appear to prove, the general factor common to the results of the school examinations and the results of temperament tests is something other than '*g*.' I suggest it is some general temperamental factor, enabling the subject to put more energy into his examination work, and incidentally no doubt to work at higher speed.

The high correlation between examinations and intelligence indicates that a general intellectual factor runs through both. This is a result that would be expected, for school examinations have been proved to be highly saturated with '*g*.' We conclude, therefore,

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[1] Downey, *The Will-Temperament and its Testing*, p. 124.

(1) that examination results and intelligence correlate on account of a general intelligence factor which we identify with Spearman's '*g*';

(2) that examination results and temperament also tend to correlate on account of a general temperamental factor which is closely related to Burt's central emotional factor[1] and may even be identified with it.

This last suggestion is offered tentatively: the T tests are not constructed so as to call into play all the various emotions the measurement of which would be necessary to obtain a good estimate of 'general emotionality.' On the other hand, there is the possibility that some factor closely related to the process of writing is operating both in the T tests and in the school examinations. This view, however, cannot afford a complete explanation of the correlation coefficients. Four tests, 4, 6, 7 and 8, involve no writing; and yet show a fairly high positive correlation with the examination results, the average coefficients being 0·393, 0·320, 0·213 and 0·238 respectively; only three of the tests involving writing have higher coefficients, while five have definitely lower coefficients. It appears clear, therefore, that the general factor is not to be identified directly with the writing process, but is some wider temperamental factor which has close affinity to 'general emotionality.' No doubt in the temperamental tests ease and speed of writing may be an incidental symptom of 'general emotionality.' This conclusion is supported by the statement of Burt, who writes: "In the few rough experiments I have made the correlation between quickness of habitual handwriting and quickness of general emotional response proves high enough to warrant tentative deductions along these lines. In a group of 143 students the correlation proved to be 0·57[2]."

We may now examine our table of coefficients to determine if the general factor which we suggest above is an adequate ground of explanation of the relations indicated by our coefficients. It will be sufficient for our purpose if we take the four tests T 1, T 2, T 3, T 4 and the two examinations E 1 and E 2, and calculate the value of all the possible tetrad differences. The following result was obtained:

Table II. *Tetrad differences.*

Tests T 1, T 2, T 3, T 4, E 1, E 2.

Mid-point of F/P.E.

±0·5	1·5	2·5	3·5	4·5	5·5	6·5	7·5	8·5	9·5	10·5	11·5
28	25	14	7	3	5	2	—	2	—	2	2

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[1] C. Burt, "General and Specific Factors underlying the Primary Emotions," *British Association Report*, 1915.

[2] "The Measurement of Mental Capacities" (*The Henderson Trust Lecture*, Oliver and Boyd, 1927), p. 20.

In a normal distribution only 45 cases should fall outside the limits of ± P.E., here 62 do so; 23 cases exceed ± 3 P.E. where about 6 cases would be expected. This distribution clearly exceeds what can be attributed to sampling errors, and indicates that, in addition to any general factor which may be present, there is a group factor or group factors operating it may be in the T tests or in the E tests, or in both. Any one of these conditions may be sufficient to account for the distribution obtained.

There appears, therefore, to be a definite indication of the existence of a group factor or factors in the T tests. We may examine this possibility by calculating all the possible tetrad differences which can be obtained from the inter-correlations of the twelve T tests. This table was given in the previous report[1], from which it will be seen that 8·7 per cent. of the tetrad differences lie outside the limits of ± 3 P.E., whereas only 0·7 per cent. should do so, and 6·2 per cent. lie outside ± 5 P.E. where we should expect only 0·1 per cent. This clearly indicates that there is correlation between the T tests other than can be attributed solely to a common factor running through them all, and thus proves the existence of a group factor or factors.

4. EXAMINATION OF THE DATA FOR THE PRESENCE OF GROUP FACTORS.

The temperamental tests are arranged by their author into three groups each consisting of four tests measuring (1) speed and fluidity of reaction, (2) forcefulness and decisiveness of reaction, and (3) carefulness and persistence of reaction. This arrangement may be regarded as implying the existence of three group factors in temperament qualities. The suggestion is further strengthened by the recurrence of the terms 'rapid-fire type,' 'aggressive or forceful type,' and 'deliberate, careful type' in Dr Downey's analysis of profiles, the types being determined by high scores in each of the three groups of tests. How far the author deliberately intended to convey such an interpretation of her grouping it is difficult to say: possibly the arrangement is merely designed for convenience of classification. Certainly, she makes no reference to group factors; and probably she intends these three groups to be understood as revealing different aspects of temperament. This seems suggested when she writes, for instance: "Profiles frequently reveal only a slight emphasis of one or two *aspects of temperament*; sometimes they are best described in negative terms as non-aggressive or careless[2]."

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[1] This *Journal*, XIX, 26.

[2] Downey, *loc. cit.* p. 71; the italics are ours.

We may examine our table for any evidence supporting a suggestion that there are group factors of speed, of drive, and of care-persistence present in the three groups of tests designed to test these qualities. The four speed tests have an average correlation of 0·232 and are definitely correlated with the exception of the slight negative correlation between Tests 2 and 3. The average correlation among the tests forming this group is higher than the average inter-correlation with the tests of the other two groups, which is 0·110 and 0·036 respectively. The tests in the aggressive group are correlated to a slight extent with a low negative correlation between Tests 5 and 8. The average correlation, however, is only 0·082. In the carefulness-persistence group Tests 10 and 11 are definitely correlated but Test 12 shows no indication of relationship to the other tests in the group. The average inter-correlation for the group, 0·075, is lower than the average inter-correlation with the tests in the speed group, 0·110, in fact Tests 10 and 11 have high correlation with the tests in this first group. There seems little evidence to support the suggestion that there are group factors underlying these three groups of tests; even in the group in which the tests appear to have the strongest tendency to hang together two of the three possible tetrad differences exceed ± 2 P.E. We may conclude, therefore, that the qualities measured by the tests of speed and fluidity, aggressiveness, and care-persistence do not represent the functioning of group factors.

From the nature of the T tests it may be suggested that any super-correlation which they may reveal is due to the considerable degree of motor ability which they involve. Investigations of the simpler motor activities, such as tapping or tracing, have failed to indicate that these activities are positively correlated[1]. This may not hold, however, for more complex activities involving freer and more highly co-ordinated movements. Burt[2], for example, as we have seen, found that writing, drawing and handwork are specifically correlated, and was led to suggest the existence of a group factor in such abilities, which he has termed a manual factor. The existence of a similar factor may be looked for in our T tests inasmuch as eight of them involve writing and another tracing. The actual inter-correlations, however, do not point to this conclusion, and it is significant to note that the correlation of the T tests with handicraft exercises is, with the single exception of test T 3, Flexibility, lower than the average correlation of each of the T tests with the two school examinations, as is seen in the following table:

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[1] Cf. Perrin, "An Experimental Study of Motor Ability," *Journ. of Exper. Psychol.* 1921, IV.

[2] *The Distribution and Relations of Educational Ability*, p. 58.

Table III. *Correlation of T tests with handicraft tests and school examinations.*

Test	Exam.	Handicraft	Test	Exam.	Handicraft
T 1	·197	·078	T 7	·213	·139
T 2	·429	·219	T 8	·239	·049
T 3	·135	·463	T 9	– ·112	– ·219
T 4	·393	·247	T 10	·429	·249
T 5	·030	– ·042	T 11	·457	·009
T 6	·320	·178	T 12	– ·202	– ·047

In handicraft motor ability and activity are definite determinants of success in the exercises worked; there is apparently, therefore, something more than a pure motor factor operating in most of these tests.

Our search for possible group factors will be assisted if we rearrange our table of coefficients as far as possible in descending order of magnitude with tests having positive correlation grouped together at one extreme and those with negative correlation grouped together at the other extreme. This may be done systematically by calculating the correlation of each test with the hypothetical general factor running through them all and arranging them in the order of these coefficients. The following table results:

Table IV. *Inter-correlation of tests.*

	2	4	11	10	1	8	5	7	3	9	12	6
2	—	·486	·322	·132	·614	·296	·158	·120	– ·083	– ·100	– ·046	– ·010
4	·486	—	·435	·275	·117	·227	– ·184	·066	·127	– ·081	·129	·168
11	·322	·435	—	·382	·308	– ·093	·087	·169	·049	·047	– ·045	– ·003
10	·132	·275	·382	—	·112	– ·006	·084	– ·039	·386	·135	– ·075	·037
1	·614	·117	·308	·112	—	·187	·173	– ·063	·130	– ·027	– ·020	– ·189
8	·296	·227	– ·093	– ·006	·187	—	– ·034	·032	– ·034	·216	– ·061	·205
5	·158	– ·184	·087	·084	·173	– ·034	—	·043	– ·153	·020	– ·033	·165
7	·120	·066	·169	– ·039	– ·063	·032	·043	—	– ·197	– ·110	·150	·083
3	– ·083	·127	·049	·386	·130	– ·034	– ·153	– ·197	—	– ·183	·075	·010
9	– ·100	– ·081	·047	·135	– ·027	·216	·020	– ·110	– ·183	—	·007	– ·142
12	– ·046	·129	– ·045	– ·075	– ·020	– ·061	– ·033	·150	·075	·007	—	– ·350
6	– ·010	·168	– ·033	·037	– ·189	·205	·165	·083	·010	– ·142	– ·350	—
Hypothetical general factor	·651	·606	·572	·488	·462	·322	·109	·087	·045	– ·076	– ·091	– ·098

We may first consider the coefficients which are apparently out of place in the hierarchical table. The coefficients which fall into this class are T 3 and T 10 0·386, T 8 and T 9 0·216, T 7 and T 11 0·169, T 4 and T 6 0·168, T 7 and T 12 0·150. An examination of the remaining correlations in relation to these indicates by the number of negative coefficients occurring that there is apparently no question of a group factor involved in the irregular coefficients arising from these tests; the explanation is probably to be found in the actual activities involved in the tests themselves. In the first case, Tests 3 and 10, this is clearly the cause as both tests involve the imitation of a model. The explanation of the fairly high positive correlation between Tests 8 and 9 is not so clearly apparent. The positive correlation is somewhat unexpected; Downey suggests[1] that Test 8, Finality of Judgment, would have negative correlation with Test 9, Motor Inhibition, though she says elsewhere that Motor Inhibition which "is given first place in the final group might be included among the aggressive traits[2]" in the previous group. Many subjects it is suggested manifest a tendency to return again and reconsider their judgments and thus spend an excessive amount of time in revision: those who score high in Test 8 have to inhibit this tendency, and the positive correlation in our table may be explained by the element of inhibition called for in working the tests. In the case of Tests 4 and 6 the latter reveals a form of self-assertion characteristic of the forceful individual who might be expected to be speedy in making a decision. Tests 7 and 12 also appear to involve somewhat similar qualities; as the author of the tests remarks: "the deliberate, careful type assert their opinion more diffidently but hold to it more tenaciously[3]." The perseverator, therefore, might be expected to reveal resistance to opposition.

We may assume, therefore, that these irregular coefficients in our table are determined by the actual test activities involved, and proceed to consider the implications of the table as a whole. There are apparently two groups of tests which show evidence of relationship. At the positive end of our table are five tests which have positive correlation with one another throughout, and are either negatively correlated with other tests or have zero or positive correlations which are too irregular to have significance for our purpose. Of these five tests, 2, 4, 11, 10 and 1, three of them, 2, 4, 1, are definite tests of speed, while Test 10 involves the rapid imitation of a model, and Test 11 is scored upon the ability to write in a restricted space in speeded time. The conclusion seems inevitable, therefore, that there is a general factor of speed entering into these tests, and the suggestion is strengthened by the fact that the highest coefficients of correlation occur between tests which are most obviously speed tests; scores in Tests 1 and 2, for instance, are determined by the speed of normal writing and the ability to speed up the act of writing. Similarly, Tests 2 and 4 measure the ability to write rapidly and the ability to make a rapid decision.

At the other extreme of our table are three tests which have an average

[1] *The Will-Temperament and its Testing*, p. 130.

[2] *Op. cit.* p. 71.

[3] *Op. cit.* p. 127.

negative correlation with one another and are, in general, negatively correlated with the tests in our first group. The main departure from the general negative correlation is the positive correlation between Tests 4 and 6 which has already been accounted for by an examination of the test activities; Test 6 is a test of non-suggestibility, and we should expect the non-suggestible type of person to score well in a speed of decision test of this nature. The suggestion is made that these tests reveal an opposite tendency to that revealed by our first group of tests. The first group of tests, to adopt Downey's titles, may be called tests of speed and fluidity (though Downey does not apply the titles to all these particular tests) and similarly the second group of tests may be regarded as tests of slowness and rigidity. The negative inter-correlations between the three tests in the group are not altogether unexpected as the tests themselves, it is suggested, throw light upon the different aspects of the slow, rigid, dogged type of person who would score high in these tests, the non-suggestible type given to "an intellectual balancing of reasons" in Test 6, the inhibited, somewhat obstinate, careful type in Test 9, and the persistent perseverating type in Test 12.

On the assumption that an approximate measure of the correlation of each test with our general factor is given by its correlation with the hypothetical general factor, a rough approximation of the specific correlations existing among our tests may be calculated[1]. Owing to the existence of group factors as well as a general factor in our variables, the figures in the following table cannot be accepted as an absolutely accurate measure of the relationships existing[2]. The figures in the following table may be used in support of suggestions made elsewhere in the paper, but, in view of the reservations stated above, which apply more definitely in the case of a table such as ours, where the presence of the general factor is not at first glance very obvious, no conclusions will be drawn from them.

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[1] For method of calculation see Burt, *The Distribution and Relations of Educational Abilities*, p. 53.

[2] Professor Burt, whose method is here followed, points out (in a letter to the writer) that it can "only be regarded as an approximation. As here applied it first assumes that the main trend of the correlations is determined by some general factor alone, and that any deviations will in the long run be neutralised as though they were due solely to chance. Having calculated the theoretical correlation with the hypothetical general factor, the method then proceeds to calculate the amount of specific correlation in isolated instances. For preliminary studies this seems to be the best procedure to use; but at the same time it can only yield a first approximation. It serves to indicate the presence of specific correlations, rather than to demonstrate their validity or measure their amount." I desire to express my indebtedness to Professor Burt for his very valuable comments upon the results of this investigation, and for his kind permission to quote from his letter.

Table V. *Specific correlations.*

	1	2	3	4	5	6	7	8	9	10	11	12
1	—	·463	·123	−·230	·138	−·162	−·025	·045	−·009	−·146	·059	·024
2	·463	—	−·071	·151.	·115	−·071	·083	·120	−·067	−·281	−·081	·017
3	·123	−·071	—	·126	−·150	·005	−·195	−·021	−·180	·419	·028	·071
4	−·230	·151	·126	—	−·317	·138	·004	·042	−·161	−·029	·135	·093
5	·138	·115	−·150	−·317	—	·156	−·034	−·001	·012	·036	·030	−·023
6	−·162	−·071	·005	·138	·156	—	·075	·183	−·135	·012	·065	−·344
7	−·025	·083	−·195	·004	−·034	·075	—	·004	−·104	−·094	·146	·143
8	·045	·120	−·021	·042	−·001	·183	·004	—	·202	−·198	−·357	−·033
9	−·009	−·067	−·180	−·161	·012	−·135	−·104	·202	—	·113	−·111	·001
10	−·146	−·281	·419	−·029	·036	·012	−·094	−·198	·113	—	·144	−·035
11	·059	−·081	·028	·135	·030	·065	·146	−·357	−·111	·144	—	·009
12	·024	·017	·071	·093	−·023	−·344	·143	−·033	·001	−·035	·009	—

The specific correlations, as here calculated, introduce some negative coefficients into our group of five tests, but it is doubtful if they are large enough to have any significance and may be due to some extent to the possible over-allowance for the general factor in the method of calculation adopted. If we apply the tetrad difference method to our group, whether we use the observed or the specific coefficients, only three of the fifteen tetrad differences are in the region of ± 3 or 4 P.E. and thus large enough to have significance, and the three cases on examination are found to be tetrads into which $r_{1\cdot 2}$ enters, while $r_{10\cdot 11}$ also enters into two of the three. Both of these coefficients, as we see from Table V, arise from tests which have high specific correlation, probably due, as we have suggested, to similarities in the test procedure in each of the two cases.

We have in our data, therefore, a group of five tests which, by their inter-correlations, reveal the presence of a group factor, and a second group of three tests which, although unexpectedly negatively correlated amongst themselves, appear to introduce factors which are negatively related to the group factor functioning in our first group of tests. We may now proceed to examine the nature of these factors in more detail and endeavour to relate them to general psychological theory.

5. THE NATURE OF THE GROUP FACTORS, AND THEIR RELATIONSHIP TO PSYCHOLOGICAL THEORY.

The results presented above apparently indicate the existence of group factors in some of the Temperament Tests under investigation; it is a more difficult matter, however, to identify the group factor in question and define it in psychological terms with any degree of precision. It follows from the nature of an investigation conducted with boys of school age that we have no introspective analysis by the subjects undergoing the tests to aid us in our efforts to interpret the results; we must, therefore, endeavour to throw some light upon the nature of the group factor by an analysis of the activities called into play in the tests themselves and the information to be derived from the correlation coefficients.

Let us consider first the group of tests which has been selected as indicating the presence of a group factor—Tests 2, 4, 11, 10, and 1, Freedom from Load, Speed of Decision, Co-ordination of Impulses, Interest in Detail, and Speed of Movement. Some indication of what is probably the most characteristic feature of this group factor may be secured by calculating the correlation of each of these tests with the pool of the remaining four tests. Using the coefficients from our table of correlations we obtain the following results[1]:

Table VI.

r_{t2}	and pool of rest	·573
r_{t11}	,, ,,	·532
r_{t4}	,, ,,	·472
r_{t1}	,, ,,	·405
r_{t10}	,, ,,	·307

From these figures we may assume that the particular temperamental quality we are endeavouring to trace is most evident in the functions involved in the tests for Freedom from Load and Co-ordination of Impulses. High scores in these two tests may at first sight be regarded as the result of organization and co-ordination of native impulses, and on this assumption the prominence in our group of these two tests may be interpreted as evidence that our group factor rests upon the co-ordination and organization of native tendencies. The method of scoring the Co-ordination of Impulses Test, it may be said, appears to emphasize the speed factor, but a subject who is merely speedy does not necessarily score high, indicating that speed alone does not characterize the quality involved. Speed is only one aspect of the reaction resulting from the organization which, in the light of these two tests, may be regarded as constituting the basis of our group factor. The bringing of impulses and reactions under control by the development of the desired co-ordinations increases the apparent amount of nervous or emotional energy by conserving it, and this increased energy may express itself in terms of speed. On the physiological side this quality may find its correlate in the functioning of the nervous system. The structures which primarily determine function on a lower level secure a higher degree of efficiency and a wider range of adaptability through co-ordination with structures on a higher

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[1] The method employed was

$$r_{(a_1 - a_2 \dots a_{p-1})\, a_p} = \frac{S_p}{\sqrt{p - 1 - S - 2S_p}}.$$

See Ll. Wynn Jones, "A Note on the Correlation of Sums," this *Journal*, July 1924, xv. 21.

anatomical plane of the nervous system. "No fresh energy is generated by this higher integration, but the response gains in freedom; it can be more closely regulated according to the needs of the moment and brought into harmony with the reactions of the organism as a whole[1]."

This interpretation in terms of psycho-neural organization may be suggested as in close agreement with the functioning of Co-ordination of Impulses and Freedom from Load in the two tests bearing these names, and as accounting for the apparent emphasis upon the speed factor which is to be understood as increased efficiency of response and not as an increase in nervous or emotional energy. The explanation upon an *a priori* hypothesis of structure and organization is not in accord with the whole of our data, as we see when we examine more closely the inter-relationship of all the tests forming our group. The central place in the group occupied by the Co-ordination of Impulses Test is clearly due to its close relationship to the definite speed tests 1 and 4, with which it has coefficients of correlation of 0·308 and 0·435. Similarly, Freedom from Load owes its position in our table to its close relationship with the definite speed tests with which it has coefficients of correlation of 0·614 and 0·486; these four coefficients include the three highest in the whole table and there are only two other coefficients which slightly exceed the fourth.

Test 2 is purely a measure of the ability to maintain speed without external pressure, the ability to keep the psychic machine running speedily and smoothly. It is significant also that the highest coefficient in our table is that between Tests 1 and 2 which are purely tests of speed in writing in its speediest or natural form. The next highest coefficient, that between Tests 2 and 4, results from tests in which speed is definitely the chief consideration. The tests which stand just outside our group of five are also closely correlated with speed. Test 8, Finality of Judgment, has a distinct element of speed inasmuch as finality of judgment is dependent upon swiftness of decision, a suggestion which is borne out by the high correlation of this test with Test 4, Speed of Decision. Test 5 indicates energy and impetuosity of reaction which again is closely related to speed of reaction, and this test also correlates with the two main speed tests. One can only conclude, therefore, that speed is a general factor operating in all the tests in our group. Speed is but one aspect of a stream of energy which may vary in volume and rate of flow. The speed of a reaction is determined by the amount of emotional or temperamental energy, the greater the amount of energy the greater the speed. We may best describe our group factor, therefore, as the amount of temperamental or emotional

[1] H. Head, "Speech and Localization," *Brain*, 1923, p. 425.

energy which is available as a result of the instinctive drives, and the speed with which that energy expresses itself.

A reference to the table of specific correlation on the whole supports our conclusion, though we find negative correlation between Tests 1 and 4. This might be expected inasmuch as Test 1 measures speed of normal writing while Test 4 does not involve writing in any form but measures the speed in assessing one's own character traits. The specific table also suggests how close is the affinity between Test 5 and our group factor. This again supports our hypothesis, for the Motor Impulsion functioning in this test is measured by the ratio of speed and size of writing under normal conditions and under distraction. This is essentially a speed quality, as rate of writing together with size is the criterion, and large handwriting is usually also swift handwriting.

Our second group consists of tests which measure qualities which, in different ways, appear to function as inhibitors of speed and fluidity arising as a result of our group factor. Test 12, Volitional Perseveration, is a test the name of which is least indicative of the quality it is testing. The term volition as here used is not to be understood in the usual sense as resulting from persistence of ideas or stimuli. Careful study of the test in practice tends to confirm the view of the author of the test that it measures some aspect of 'psychic momentum.' There is no objectively established end for the task involved in the test, which is probably tapping a real perseverative tendency. The perseverator is one in whom the speed of flow of energy is slowed down; thus Lankes found[1] "that the perseverator on the whole tends to be slower than the non-perseverator." We should expect a perseverating person to be handicapped in a speed test, and to this factor we should attribute the "semblance of slowness" which Bernstein says[2] was given by "temperamental peculiarities." In Test 9 Motor Inhibition is measured by the extent to which the subject is able voluntarily to retard the time taken to write a given phrase. This is purely a conscious effort to check the speed of discharge of energy measured particularly by Tests 1, 2 and 4, with which it has negative correlation. This quality may be explained as the power to check the discharge of one impulse through blocking by the enforcement of another impulse. Suggestibility, measured by Test 6, Downey has found to be "interwoven with motor expansiveness or impulsion[3]," and, therefore, the suggestible person would be likely to excel in our speed tests: the non-suggestible, who score high in this test, we should expect to be more rigid and slow in reaction.

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[1] W. Lankes, "Perseveration," this *Journal*, 1915, VII.

[2] *B. J. Psych.* Mon. Suppl. 1924, VII, 21.

[3] *Loc. cit.* p. 115.

The negative correlation between these three tests is due to the fact that they represent different aspects of a process of inhibition which hinders nervous energy expressing itself immediately in motor reaction. Our definite negative correlation between Tests 6 and 12 is supported by the only investigation carried out up to the present which has any bearing on this question. Aveling and Hargreaves found[1] no tendency for suggestibility to be correlated with perseveration. Although negatively intercorrelated it is suggested that the qualities involved in these tests may function as a unitary trait however complex and varied it may be in origin.

The best psychological explanation of the results of our analysis of the data obtained in this investigation is on the general lines of Burt's theory[2] of general emotionality. This theory is best stated in his own words. "If the meagre evidence can be trusted, there seems to be, on the temperamental side as on the intellectual, a central or general factor. This may be termed 'general emotionality.' In addition, there appear to be at least two fairly broad special or group factors making the one for a repressed disposition, and the other for an unrepressed[3]." We have indicated above the existence of a general factor running through our tests of temperament qualities and this we suggest identifying with general emotionality. Our two group factors are best explained as due respectively to the predominant strength of the 'sthenic' or aggressive instincts (anger, self-assertiveness, curiosity, sex, and the like) producing an unrepressed temperament, and of the inhibitive group of instincts (fear, disgust, submissive tenderness, and the like) producing a repressed temperament. We thus reach the conception of two temperamental sub-types—"(i) the aggressive, and (ii) the inhibited respectively[4]."

This interpretation is in agreement with modern psychological theory. McDougall holds that "the essence of instinctive activity seems to be liberation and direction of energy[5]," and suggests that the most satisfactory position with reference to the fund of energy assumption is a combination of the two theories, one which postulates a common fund of energy for all the instincts, and the other which assumes the existence of a non-interchangeable amount of energy attached to each instinct.

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[1] "Suggestibility with and without Prestige in Children," this *Journal*, 1921, XII.

[2] *Brit. Assn. Report*, 1915, Section I, pp. 694–7.

[3] C. Burt, "The Measurement of Mental Capacities" (*loc. cit. sup.*).

[4] *Idem*, "The Definition and Diagnosis of Moral Imbecility," *Brit. Journ. Med. Psychol.* 1926, pp. 30–3.

[5] *An Outline of Psychology*, 1923, p. 107.

This provides a theoretical foundation for our conclusions regarding a general factor and group factors.

The classification has some features in common with Trotter's pair of contrasted types, the stable minded and the unstable minded[1]. The former type is energetic and active with "well-defined conative channels allowing of the strong and smooth outflow of psychic energy[2]" corresponding with our unrepressed temperament; the latter type is lacking in psychic energy or fails for some reason to give the energy effective expression thus corresponding to our repressed temperament.

The two group factors, it has been suggested, are related to the traditional distinction between two contrasted types, recently revised and popularized by Binet and by Jung. Jung's psychological types[3] are determined by the direction which the psychic energy or libido tends to take spontaneously; in the extravert (or objective type, as Binet termed it) the psychic energy is directed outwards, while in the introvert (or subjective type) it is turned inwards. The former type corresponds to our unrepressed temperament in which there is a free and unimpeded flow of nervous energy; while in the latter type the energy fails to find its natural outlets or is inhibited in some way, as in the case of our repressed temperament.

An interesting question arises when we compare the results of this investigation with the work of Webb[4] in which he puts forward the hypothesis of a general factor 'W' in character qualities, which is "in some close relation to persistence of motives." This general inhibitory factor is commonly regarded as positive in nature. Among recent writers Roback, for example, who regards inhibition as the basis of character, writes "Inhibition is thought to be a negative process. In reality, however, it is a positive force[5]." If this hypothesis were true our repressed temperament should have revealed the presence of a positive group factor and our table of observed correlations should be reversed. We can only conclude that our results fail to confirm the assumption that inhibition is a positive process. Professor Burt puts the matter very clearly in a letter to the writer. "I see in this result some confirmation of my theory of general emotionality and of two subordinate groupings of the specific instincts. Most earlier writers have been tempted to describe the emotional person as suffering from a lack of inhibition or control. They have taken inhibition as a positive factor. To me the emotionality is a positive factor; inhibition I regard as a negative process, the secondary result of many possible factors, different in different cases. If there were a central factor of inhibition, then we would expect the latter group of temperamental tests to reveal a general factor. On my theory, however, emotions may be slowed down or inhibited, in various ways—by each other, or by higher sentiments, or by acquired habits; their 'fluidity' may thus be impeded by a number of different factors which are not necessarily related amongst themselves. This seems confirmed by the presence of a negative rather than a positive inter-correlation among tests 6, 9 and 12 in your series[1]."

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[1] W. Trotter, *Instincts of the Herd in Peace and War*, 1923: "They (the unstable) tend to be weak in energy.... Thus we see society cleft by the instinctive qualities of its members into two great classes" (p. 59), etc.

[2] A. G. Tansley, *The New Psychology*, 1922, p. 104.

[3] C. G. Jung, *Psychological Types*, 1923: "Extraversion means an outward turning of the libido" (p. 542). In the introvert "the disposable energy is withdrawn into the self" (p. 145).

[4] E. Webb, *Character and Intelligence*, *B. J. Psych.* Mon. Suppl. I, p. 60.

[5] A. A. Roback, *The Psychology of Character*, 1927, p. 455.

In view of the relationship of our two types of temperament to the introverted and extraverted types it is of interest to quote McDougall's view that "It is uncertain whether introversion or extraversion should be considered a positive quality[2]." This appears to leave the question under consideration a problem that awaits solution. Tansley, however, writes: "The process of extraversion is the primitive biological function of the mind," which appears to imply that extraversion is a positive quality, and is in general agreement with the view formulated as a result of this investigation.

Within the limits of validity of this investigation we have identified a general factor and group factors in temperamental qualities. Further investigation of the problems underlying these conclusions is necessary to determine whether our factors cover all activities or whether they function only in the activities called forth by the stimuli presented in the conditions set down in the investigation. A recent writer who has analysed the conclusions of a number of psychologists who have studied temperament, while failing to find any universally admitted factor in temperament, ends her theoretical analysis of their work with the conclusion that "further efforts should be conducted on the lines of investigating differences in speed of reaction and in duration of maintenance of the intital level of activity[3]." The results of this investigation indicate the need for work in a somewhat similar direction, and emphasize the importance of relating the work to the instincts, which McDougall has termed "the prime movers of all human activity...and in them we are confronted with the central mystery of life, mind and will[1]."

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[1] (In a letter to the writer.) For a discussion of inhibition as a central factor see Burt, *Brit. Journ. Med. Psychol.* VI (1), 18–21.

[2] *Loc. cit.* p. 358.

[3] C. Bloor, *Temperament*, 1928, p. 152.

The conclusions reached in this investigation are necessarily tentative, as sufficient evidence is not available at present on the subject to warrant any finality or definiteness of statement. The tests used are certainly bringing to light innate qualities and the group factors isolated apparently have significance. Working on these lines we may hope to throw some light upon the problem of Temperament in the solution of which so little of definite value has yet been accomplished. The need is clear "for the problems of temperament and character constitute one of the most difficult and urgent provinces for future psychological research[2]."

6. SUMMARY AND CONCLUSIONS.

We may summarize our results by stating briefly the conclusions which, apparently, may be drawn from the investigation:

(1) There is a general temperamental or emotional factor entering into the T tests which is closely related to general emotionality and may provisionally be identified with it.

(2) There is little if any direct relationship between temperament as measured by these tests and intelligence.

(3) (*a*) Examinations and intelligence correlate on account of a general intellectual factor which we identify with Spearman's '*g*.'

(*b*) Examinations and temperament correlate on account of a general temperamental or emotional factor. This will account for the failure of some boys of high intelligence to do well in school examinations.

(4) There are apparently two group factors operating in the qualities involved in the T tests.

(*a*) One group factor manifests itself in the speed and ease with which temperamental or emotional energy is allowed to express itself.

(*b*) The other group factor functions as an inhibitor of the speed and fluidity with which the energy can find vent.

(5) These two factors tend to yield an unrepressed 'type' or temperament and a repressed 'type' or temperament, determined, it is suggested, by the relative predominance of the aggressive group of instincts and the inhibitive group of instincts respectively.

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[1] *Social Psychology*, 13th ed. p. 44.

[2] Consultative Committee—Report on "Psychological Tests of Educable Capacity," p. 60.

(6) Emotionality, it is suggested, is the positive factor, and inhibition a negative process resulting from several factors which are not necessarily related amongst themselves.

(7) Our general temperamental or emotional factor is identified with the emotional energy underlying all the instincts, and our group factors are determined by the subordinate groupings of the instincts with their specific energies.

The investigation, therefore, confirms Burt's theory of general emotionality with sub-factors due to the aggressive and inhibitive instincts, and is in agreement with McDougall's view of the common fund of instinctive energy and non-interchangeable energy attached to each instinct.

(*Manuscript received* 4 *March*, 1929.)

NOTES ON THE EMOTION OF FEAR AS OBSERVED IN CONDITIONS OF WARFARE

By F. AVELING.

(*From the Psychological Laboratory, King's College, University of London.*)

1. *Sources of information* (pp. 137–138).
2. *The nature of fear* (pp. 138–139).
3. *The antecedents and conditions of fear* (p. 139).
4. *The immediate effects of fear* (pp. 140–141).
5. *The after-effects of fear* (p. 141).
6. *The control of fear* (pp. 141–144.)

The following 'Notes' were compiled at the request of a colleague for a collection of psychological observations with relation to military activities. They were written at La Lovie in the summer of 1917; and only recently found their way back to the hands of the author, with a suggestion that they might be worth publishing. They are accordingly offered, with some academic diffidence, almost exactly in the form in which they were originally put together, such slight modifications only being made as seemed desirable in order to adapt them to that of an article in the *Journal*.

1. SOURCES OF INFORMATION.

With the exception of a certain number of cases suffering from the after-effects of extreme terror (possibly also complicated with true shell-shock) which he kept for a short time under observation, the writer was able to undertake no experimental research in which exact measurements could be made and from which statistics could be derived. He is able, therefore, merely to advance a certain number of generalities, based upon personal observation, introspection and retrospection, as well as upon a number of relations of officers and other ranks whom he questioned with regard to the subject. Although there is much omission, for the purpose of this paper a certain amount of psychological commonplace has been included; but at the same time there are certain observations which appear to be new, or at any rate newly emphasized in the conditions of actual warfare. Occasions for the eruption of fear were certainly not lacking; and opportunities for the exercise of self-control were as frequent as were the occasions tending to disequilibrate emotional stability. The descriptions given to the present writer by those whom he consulted, whether in the relatively fixed units such as Army Corps or Headquarters, or in Rest Camps or Casualty Clearing Stations, as to the experience of fear itself, the character of the mental processes both emotional and intellectual under the influence of fear, and the effect of other spontaneously arising or induced mental factors upon fear and self-control, were remarkably unanimous, allowance being made for individual differences of mental temperament, for the physical circumstances in which the fear complex arises, and for the physiological state of the subject at the time.

2. THE NATURE OF FEAR (DESCRIPTIVE).

1. Fear is a simple, instinctive emotion, impossible to define or directly analyse. It is susceptible of degrees, and is most easily characterized by its psychical and physiological effects. (*Cf. infra*, Section 4.)

2. As an emotional state, it tends to pervade the entire field of consciousness, whether it be weak (nervousness or apprehension), strong (fear), or dominant (terror).

3. It has no contrary emotion, with which it could be represented graphically by a single line with an 'indifference point'; but it can be represented by a line originating at zero and rising to a point at which complete mental disequilibrium (unconsciousness, insanity, etc.) occurs.

4. Together with the course of the emotion of fear, there is present in consciousness an evolution of cognitional processes (percepts, ideational and imaginal contents, etc.) which apparently act upon and are reacted upon by the emotional course. (*Cf. infra*, Section 4 (*b*).)

5. There are also generally present to consciousness feelings of psychical strain or tension, unpleasantness and excitement; as well as bodily sensations, especially muscular (notably heart), visceral and respiratory, together with sensations from and consequent upon changes in secreting glands (salivary, sweat, etc.).

6. Many, if not all, of these feelings and sensations can be discovered by introspection during the course of development of a fear crisis. They can generally be retrospected and recognized with comparative ease by any observer, trained or untrained.

7. The eruption of fear in consciousness is usually, though not invariably, abrupt; though the course of its development from weak to strong, or *vice versa*, may be observably of very considerable duration. The termination of the course may be gradual or abrupt.

8. Besides being highly unpleasant and distressing, fear as a mental state and on account of its physiological repercussion unfits those who suffer it, to a greater or less extent, for the performance of whatever task or duty they may have in hand. The more or less complete disequilibrium of fear varies inversely with self-control.

3. THE ANTECEDENTS AND CONDITIONS OF FEAR.

1. Fear has as its cause (antecedent) an objective content of consciousness either perceptual, imaginative or ideational. With this is connected either the consciousness of a relation, or a judgment, or belief that the object or situation involved is formidable (threatens in some way the subject's well-being).

2. There would seem to be enormous individual differences in the matter of susceptibility to fear, varying from almost complete fearlessness to extreme predisposition to terror and panic. It is obviously impossible to compare or contrast fear in different individuals with regard either to its quality or quantity. At most, judgments could be made on the grounds of comparison of bodily actions caused by fear; and such judgments would be utterly untrustworthy on account of other disturbing factors (as self-control, etc.) intervening. In different experiences of fear, however, on the part of the same individual comparisons and contrasts can be made; and subjects consulted bore the present writer out in considering certain conditions as predisposing to fear. Here again factors other than those to be noted may have their effect on the fear arising in any given situation, emphasizing or neutralizing the influence of these predisposing conditions. Nevertheless, a certain number out of what in all probability is a very large number of them may be noted; as, for example, (*a*) *Psychical Antecedents:* morose brooding on thoughts of fear; evil conscience; awareness of fear in others; sense of isolation; psychical fatigue; insomnia (caused, *e.g.* in warfare by sound of guns, etc.); unfamiliarity of situation; associations between things or places with previously experienced fear; (*b*) *Physiological Antecedents:* general state of health, lowered vitality, etc.; hunger (up to a certain point only); muscular fatigue (also only to a certain point); indulgence in alcohol, sexual abuse, etc.; lack of physical occupation, idleness, etc.; (*c*) *Physical Antecedents:* asthenic conditions, as dull, bad weather; darkness (night); unfamiliarity of place; evidences of previous formidable activity (often offset by curiosity, etc.).

4. THE IMMEDIATE EFFECTS OF FEAR.

Whatever view may be taken as to the relation of the emotion of fear to the physiological manifestations of it, it would seem to be indubitable that the bodily changes which occur have an extremely large part to play, at least in intensifying the emotion. The great mass of feelings and sensations, to some of which reference was made in Section 2, note 5, come streaming into consciousness, some of them more or less distinct, others vague and obscure. Many of these sensations are unusual, or unusually intense. Their simultaneous presence in normal life is exceptionally rare. The great amount of nervous energy that is liberated has its psychical counterpart in the mental energy liberated and the psychical changes that take place. Of these the intensifying of the emotion is one, whether because of the liberation of energy upon which it feeds, energy which sustains and increases it; or whether the actual bodily state is in itself formidable. However this may be (the writer is inclined to think that both may be true) the following are some of the phenomena which should be classed under the heading of "Effects of Fear."

(*a*) *Physiological effects* (introspected). These are well known. Included among them are: phenomena of attention; innervation of the eye-muscles, with fixation of the eyes; of the neck and shoulder muscles, with rigidity of posture; of other skeletal muscles governing posture (introspectively the two first named are the most pronounced). When trembling, etc., mentioned below, occur involuntarily, there is generally a conscious effort to innervate these muscles in order to secure rigidity. There is often a quick retardation of respiration, followed by a rapid and shallow breathing. The heart-beat seems to stop, and then to be much accelerated. Changes in the secretion of certain glands occur. Most noticeable among these are the salivary (retarded or arrested); sweat (palms of hands, forehead, etc.); in some cases, lachrimatory. (These are all, as a rule, present to consciousness.) Changes take place in the sympathetic nervous system. There is relaxation of sphincters; general feeling of sinking in pit of stomach; desire to urinate, etc. Contraction and relaxation of skeletal muscles occur; trembling, teeth chattering, etc.; sometimes spasmodic choreic movements, cries, etc. Later, there is lassitude and fatigue.

(*b*) *Psychical Effects*. Feeling of helplessness (consequent on physiological effects of fear); feeling of tension ('tending toward'; a characteristic phenomenon of attention); feeling of aversion; narrowing of the field of attention, with (?) heightening of intensity of its contents;

quickened, or accentuated, processes of thoughts, judgments, etc.; in more extreme cases confusion of thoughts, judgments, etc.; enhanced vividness and liveliness of imagination; quickened judgments and decisions; or retarded judgments and decisions; vague mass of sensation, from which individual sensations may stand out with prominent emphasis; in extreme cases total loss of self-control, unconsciousness, insanity, etc.

5. THE AFTER-EFFECTS OF FEAR.

Certain of the effects of fear tend to persevere in some cases after the emotion as such has disappeared. There are extremely interesting cases in which one or more of the physiological manifestations (effects) as, for instance, tremor, local anaesthesias, aphasia, amnesia, etc., endure long after the formidable object, or circumstance, has ceased to exert any direct effect upon consciousness. These subjects, who present the ordinary symptoms of hysteria, are very susceptible to suggestion and, in the vast majority of cases, are easily cured of their trouble and its manifestations byit. It would appear that in such cases a 'fear complex' has become buried in the subconscious mind, of which the effects persist after the manner of effects of unconscious wishes. This is a subject which, in the opinion of the present writer, would well repay careful investigation. Doubtless many other internal secretions are stimulated, or retarded, as those of the thyroid, adrenal and hepatic glands (stimulation); and effects brought about such as hyperacidity of blood, and the like.

6. THE CONTROL OF FEAR.

Exact psychological information with regard to normal mental processes would suggest the employment of certain more or less obvious methods as useful in the maintenance of self-control in the presence of situations likely to induce fear, or in recovering it once disturbed by fear; and, as a matter of fact, several of such methods have been found by experience to be useful. Some of these methods are not of a purely psychological character, but are here included because of the indirect effect they produce upon the emotion itself. In the majority of the methods indicated more than one psychical mechanism is evidently employed, of which the chief would seem to be: direction of attention; substitution or suppression of motives; draining away of neural and psychical energy; transfer of psychical energy from one motive to another; alteration of the physiological effects of the emotion which tend to keep it in being and increase its intensity.

The chief methods indicated are the following:

1. Drugs, alcohol, etc. These may have a more or less immediate beneficial effect; but the subsequent effects are favourable to the genesis of fear.

2. Bodily activity. To be actively engaged upon some form of work helps in the suppression of fear and the maintenance of self-control. This is probably due to more than one factor. There is a draining off of the nervous energy through the channels of the nerves supplying the muscles involved in the performance of the work. There is also distraction of attention (cf. *infra*, 5). The process of draining off energy in this case may be compared to that of the curious tricky habits of some public speakers (twirling watch-chain, fiddling with eye-glasses or the button of the coat, etc.; of which it has been plausibly suggested that the draining of energy inhibits the ready emergence of irrelevant associated and free ideas, and thus assists a clear and logical sequence of thoughts connected with the topic dealt with).

3. Breathing, deep and regular. To a certain extent this method can be consciously employed with success. It would appear that such breathing has its repercussion on the circulatory and perhaps other systems, thus tending to counteract some of the unusual and irregular physiological activities of which mention has already been made as increasing the emotion (cf. *supra*, Section 3, 1).

4. Singing, especially chorus singing, aloud. The rhythm makes for good breathing. Attention is distracted. A sense of gregarious solidarity which tends to inhibit fear is set up. Probably also the tune and words of the song arouse associations which drain off energy from the emotion.

5. Distracted attention. This would seem to be one of the most efficacious methods of controlling fear. It is obviously, however, difficult to distract attention in circumstances occasioning strong emotions merely by voluntarily attempting to alter its direction; and this is especially true while the subject is experiencing the emotion of fear. Nevertheless, the desired effect may be brought about in various indirect ways, thus providing a motive for self-control. The subject may occupy himself with some manual work or physical labour. He may receive a command from a superior officer which he must obey, or concerning which he must think actively. Cases have been observed in which, under the influence of fear, an objective content or representation has arisen spontaneously or by association in consciousness, to which a new emotional tone has attached. The writer does not know of cases in which it has been entirely inhibited. Religious considerations which arise voluntarily or spontaneously in fear crises no doubt also bring this mechanism into play.

6. Feeling or sense of responsibility. This would apparently involve the mechanism of distracted attention, at the same time providing a powerful motive for self-control.

7. Human respect; shame of appearing to be afraid; possibly also (though the present writer does not possess very strong evidence for this) thoughts or representations of the penalties of giving way to fear and cowardice. Considerations of this nature bring the self-centred and self-seeking feelings into play. These are strong feelings; they constitute a strong motive, and involve draining and transfer.

8. Tranquil conscience. This may be secured in various ways, chiefly by religious and moral means.

9. Imagination and instinctive false judgment. The practical instinct to take cover manifests itself in greater confidence and decrease of fear even when cover is totally inadequate and worse than useless. Any imaginary cover is better than none so far as the emotion is concerned.

10. Emotional conflict. It is possible to decrease, and even totally to inhibit fear by securing its conflict with other emotions, as anger, hatred, etc. Fear may change abruptly to anger or hatred, following on the emergence in consciousness of a presentational or imaginative content, for which the latter, in the totality of the given circumstances, is an appropriate emotion. This may also be substituted for the former by the use of an intermediary idea implanted from without, as well as by even trivial irritating circumstances occurring during the experience of fear. But at the same time it must be noted that fear and anger, at any rate in their more feeble forms, can co-exist simultaneously in consciousness.

11. Suggestion. Predisposition to fear attacks is somewhat lessened by the use of suggestions given in the hypnoidal state. The writer has no data with regard to suggestions given in deep hypnosis. But, in the case of subjects readily disposed to fear, the duration of the persistence of hypnoidal suggestion seems to be limited; and fresh occasions of fear tend to break down easily the implanted 'self-control,' 'self-confidence,' 'strength of will,' etc., complexes. Simple suggestion during a fear crisis is often distinctly beneficial. The mere presence of others who are collected and calm helps subjects inclined to fear to preserve self-control. Sometimes a superstitious belief in the luck of visible and tangible objects, as mascots and charms (contagious magic) acts as a species of suggestion or auto-suggestion with appreciable results. Prestige, both professional and personal, is of large importance in the use of methods of suggestion.

12. With regard to the removal of mental and physiological effects

of the symptoms of fear (shock) which persist when the actual emotion is removed, these have been treated with remarkably good results by simple suggestion, suggestion with faradization, hypnoidal suggestion and hypnosis. Individual cases vary enormously; and the particular method best to be employed in each must be determined by the neurologist or psychologist. Tremors, local anaesthesias and sensory disturbances, etc., are generally amenable to the first three methods named; amnesia is most easily treated by the third and especially the fourth. Patients are exceedingly susceptible to suggestions of all kinds connected with their malady, or supposed malady; and the greatest care should be taken in diagnosis and treatment not to induce ideas and symptoms by injudicious questioning, manipulation, or in any other way which might lead the subject to imagine them.

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SOME SUBTLER SKILLS

By T. H. PEAR.

(*From the Department of Psychology, University of Manchester*[1].)

A definition of skill (pp. 145–146).
Skill and ability for deliberate, adequate, verbal expression (pp. 146–147).
Speech habits (pp. 147–149).
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A visualizer's attitude towards the 'verbalization' (sic) *of his thoughts* (pp. 152–153).
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Skill, technique and decency (pp. 156–158).
The stylization of subtle skills (p. 158).
The fate of skills that are left alone (pp. 158–159).
Prevalent attitudes towards important skills (p. 159).
The social meaning of these considerations (pp. 159–160).

In recent publications[2] I have suggested an analysis of skill. From those treatments the subtler human skills were exempted. The present paper will sketch a tentative analysis of some of them.

A DEFINITION OF SKILL.

In the writings mentioned, skill was treated as behaviour and as experienced 'from inside.' It was defined as (*a*) the integration of well-adjusted responses, and (*b*) the kinaesthetic knowledge which is the immediate concomitant of such behaviour.

It is supremely important to distinguish between kinaesthetic, or stataesthetic, knowledge, obtained through the locomotor apparatus, and that which has arrived by way of the eyes, ears, tongue, nose, etc.[1] In practice, obviously, some persons may find it unnecessary to distinguish them. Yet this fact does not justify such a confusion in psychology. A person who has learnt by heart the best prescription for an omelette may not make one as well as an illiterate peasant. Applied science continually demands a distinction between these classes of knowledge; *e.g.* the 'eye-ear' knowledge of chemical processes which even a paralysed man might have, and the skill in causing certain chemical changes, which might be the pride of an 'ignorant' workman. In medicine the deliberate divergence of knowledge and skill is too well known to require emphasis. Yet in discussions of industrial psychology such a distinction still appears to surprise many practical persons.

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[1] Substance of a paper read to the British Psychological Society on 9th March, 1929.

[2] *Fitness For Work*, London, 1928, Chapters I, II and VI; "The Nature of Skill," Presidential Address to Section of Psychology, British Association for the Advancement of Science, Glasgow meeting, *British Association Addresses*, 1928; "The Nature and Present Position of Skill in Industry," *The Highway*, London, Feb. 1929.

SKILL AND ABILITY FOR DELIBERATE, ADEQUATE, VERBAL EXPRESSION.

I have already emphasized[2] that while in some people these two abilities co-exist in high degree, in some they do not, and in still others they appear to be almost inversely related. Obviously, correlations between these or any other abilities cannot be predicted without scientific investigation. But since, while some skills are richly endowed with expressive words, others are not, it is clear that, in relation to certain skills, few people will be found possessing both these abilities in a high degree.

Following a remark of Mr Bertrand Russell's in his paper on "The Meaning of Meaning[3]," I proposed that this ability for deliberate, adequate, verbal expression should be termed 'intellect.' The suggestion has not aroused enthusiasm. I am therefore desirous of finding a word to mark off this important ability. For important it is though nameless, and like some nameless people, by that fact, embarrassing.

The phrase 'speech habits' expresses exactly what this word must *not* mean. I do not propose to use for it a letter of the alphabet. To encourage this tendency runs the grave risk of a newer 'new psychology' with twenty-six faculties.

I should like to lay stress upon 'deliberate' and 'adequate' in the phrase at the head of this section. Having been asked whether, if an under-tipped taxi-driver fluently reviled his 'fare,' he would be using his intellect, my answer is that in and so far as he is using speech habits, he is not intellectual. But in and so far as he is deliberately collecting, discriminating and choosing his linguistic gems that they may precisely describe his attitude, he is intellectual. Or, of course, whatever better adjective may be found to describe this quality.

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[1] Cf. *Remembering and Forgetting*, London, 1922.

[2] *Fitness For Work*, Chapter II; *Highway*, *loc. cit.*

[3] *Mind*, N.S. vol. XXIX, Off-print No. 116.

SPEECH HABITS.

I should welcome a temperate use of the term 'speech habit,' to denote a specific response to a specific situation, bearing the other mark of habit: automaticity. Such a term would mark off from the immediate products of thinking so many expressions which, once the product of thinking[1], are now mere habits.

Speech, of the kind which enters into forensic skill, may consist of habits. They, however, may appear to the uninstructed listener to be fresh utterances. A hardened committee-man, opening his attack with "Unfortunately I do not possess the technical knowledge...," occasionally means just that. Often he implies, "I have, however, the robust common-sense which is better than the expert's crankiness," or, "I have the penetrating intelligence which makes technical knowledge superfluous." He may even emit these habitual phrases in a bored way, as a fencer may languidly parry the strokes of a weak opponent.

While complicated modes of speech may be habitual, simple modes of speech obviously need not be habit.

The spectator of a skill may care little if it be habit or a novel production. For a psychological observer the difference is supremely important. Even the statement that Art is to conceal art—a supremely skilful series of words—warns against the simple-minded muddling of skill with habit. Many pupils of a teacher of skill whom I know display an apparent careless 'natural' ease. That their teacher presents an unfailing example of this light touch is not without significance. What therefore appears to the spectator as a pattern of easy habits may be a technique, acquired by imitation, analysis, thought and assiduous practice. (One easy way to discover truths about this is—curiously enough—to ask the performer.)

The use of the term 'habit' by some behaviourists.

Recently some American psychologists Divorced Old Barren Instinct from their Bed. Their writings now suggest that they Took the Daughter of the Vine to Spouse. For what they—their critics too—would call their speech habits have rapidly regressed from epicritic to protopathic. There is a lack of discrimination between blatantly different things, and a coarsening in graduation of response. This is not all. It is a tenet of science, and not less of the Victorian Fundamentalist physical science to which some behaviourists appeal, that having discriminated things one should henceforth call them by different names. Yet when the word 'habit' is made to stand for—or eclipse—sensory discrimination, will, ability, art, intelligent adaptation, routine adaptation, independent judgment, inventive imitation, moral behaviour, thought, sensitive perception, flexible response, and even the self, the psychologist's world becomes a fog, in which, notoriously, things appear alike.

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[1] For a discussion of the nature of thinking see Henry Head, *Aphasia and Kindred Disorders of Speech*, London; F. C. Bartlett, "The Relevance of Visual Imagery to the Process of Thinking," this *Journal*, 1927, XVIII, 23; T. H. Pear, "Is Thinking merely the Action of Language Mechanisms?" this *Journal*, 1920, XI, 71.

It is sadly true that the meanings of words get worn with use. But the quickest way to wear the edge off any word is to use it for many different things. This habit, attributed in our own country to the schoolgirl, should be discouraged in adult psychologists writing in any language.

Subtle skills.

Most of the skills I have taken as examples in previous writings have been relatively unsubtle. Skills which we like watching in others usually lend themselves to 'behaviouristic' study. Most of them involve chiefly the larger muscles, though striking exceptions in some countries are speech and song. Is there, however, difference of *kind* or only of degree, between simple and subtle skills?

I believe that the difference is one of degree; yet, since the chief components of the subtler skills are shyer and more elusive, they are apt to be avoided or passed over by the practitioners of the behaviouristic method.

I may perhaps illustrate this by taking as an example the technique aimed at in the special art of the 'radio talk.'

To prepare and to give one, as distinguished from merely reading passages composed in written style—which may be magnificent but are not a 'talk'—is to aim at a new set of skills. The technique requires collection of possible, followed by choice of suitable, matter. Such choice demands discrimination with an aim in view. This aim may be difficult to fix upon, since the composition of the audience is a matter for speculation. There must be consideration and arrangement of the order in which the points are to be presented, and of the relative stress, psychological, aesthetic and acoustic, which they will receive. Since, at the moment of delivery, moods and 'overtones' of meaning can be conveyed by vocal skill alone, the expert talker may mentally rehearse his intonation, even marking his manuscript accordingly. That special speech-device made possible by the microphone, the trailing-off of the voice, like the 'fade-out' on the screen or the dimming of a stage; the hesitations, even the words found after the hesitation, may be deliberately planned and rehearsed. To describe it will seem to many as indecent as the uncovering of any other technique, a theme which will presently be discussed.

This technique therefore owes much to artifice, whatever that word when analysed may mean for psychology. This is also true of the subtle skills, shown by a lawyer or a hostess, foreseeing and avoiding possible *contretemps*.

THE PRELUDE TO SKILL.

I would suggest that it is profitable to consider anticipation, collection and selection, as the prelude to skill. For one who has acquired taste and discrimination in a purely verbal or bookish way, may be able to imagine how he would act skilfully and yet be clumsy in his actual dealings with others. Usually the extravert displays real social skill; the introvert doing it in his dreams. And skill in dreams appears to be an untouched subject.

In art and sport these distinctions are clear and accepted. Wimbledon attracts many persons, whose taste and discrimination permit them to appreciate the fine points of a tennis stroke which they will never make. Some stage dancers, who possess great skill, fortunately rely for taste and discrimination upon their artistic directors.

The first part of our definition of skill, the "integration of well-adjusted responses," might be accepted by a behaviourist, though the word 'integration' may suggest trouble for him. Less convenient for him is the second part, implying that skill involves power and *knowledge*. For an essential in many skills is fore-knowledge or anticipation.

ANTICIPATION AND INTUITION.

Skill and anticipation.

The relation of skill to anticipation raises a problem of great theoretical significance. High-grade skill, whether in a motorist or in a lawyer, implies ability not only to avoid actual obstacles, but to 'foresee' them. I fence this word by inverted commas to make it clear that that seeing, actually or in imagination, is not the only method of anticipation.

By what processes are obstacles anticipated? In some persons verbalizing seems pre-eminent. But, when one is drafting the presentation of a proposal so that it shall meet with fewest objections from a committee, visual imagery of the expressions or actions of the members may play an important part. In some of us, as I have urged elsewhere, it is too insistent and ubiquitous to be denied significance, or relevance[1].

Skill and intuition.

The use of intuition as an explanatory principle has diminished lately. A possible reason, apart from the term's vagueness, is the rapid incursion into our daily life of mechanical inventions. These present problems to solve which scientific information is indispensable. A person may intuitively judge that a child, who, a moment ago, was crying merely to attract attention, is now expressing pain. But no such gift will tell him the cause of microphonic noises or 'motor-boating' in his wireless set.

By some, the term 'intuition' is restricted to signify an adequate unlearned interpretation of a biologically important situation, the essential features of which have often occurred in the history of the race, though not in that of the individual. This use of the term would raise all the intricacies implied in the first part of Professor W. McDougall's definition of instinct, "an innate psycho-physical tendency to *perceive*... objects of a certain class[2]," and all the problems of *Gestalt*.

The question whether man possesses any inherited skill is difficult. At birth he is unskilled. But the absence of skill in very early life may be due in part to the necessity for appropriate maturation[3] of the bodily apparatus. Experience is also necessary to form the setting, the background and the provocation for skill.

VERBAL DESCRIPTIONS OF VISCERAL EXPERIENCE.

To my mind the most interesting and helpful of the behaviourist's conceptions, invoked to explain thinking, is that as education progresses visceral experiences may become 'verbalized.' (We enter a *caveat* here against this use of the word 'verbalize,' returning to the subject on p. 152.) Many psychological explanations which nowadays involve an 'unconscious,' may be possible in terms of visceral and muscular experiences to which their possessor has given no names. Accordingly, they have not been focussed clearly against their unanalysed background[4].

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[1] "The Relevance of Visual Imagery to the Process of Thinking," this *Journal*, 1927, XVIII.

[2] *Introduction to Social Psychology*, London.

[3] Cf. K. Koffka, *The Growth of the Mind*, London.

[4] This should not allow us to forget that many insistent intrusions of the 'unconscious' take the form not of muscular and organic sensations, but of visual, auditory and other images (cf. *Remembering and Forgetting*). This fact will not be forgotten by doctors, artists and writers, but is apt to be ignored by behaviourists.

Such 'verbalizing' of visceral experience may play two important parts, both in the exhibition and the inward realization of the subtle skills.

(1) Certain techniques which are nowadays supposed to be, in great part, 'unconscious,' might be made more conscious, or even completely conscious. (This idea will be developed on p. 155.)

(2) By being brought to consciousness, the unverbalized organic and muscular experiences which now add to certain skills an undertone of emotion (sometimes weak, sometimes overwhelming) might lose their mystery. Their consequent 'subterranean' effect might then cease to hamper the skill.

From experience I know that not a few athletes who, in the games they play, have fallen many times, admittedly fear falling on ice. Their fear is often based upon wrong mechanical, anatomical and other ideas with their accompanying actions. It is decreased when the ideas are corrected, and the learners are shown how to fall, how not to, and why.

No psycho-therapist doubts that talking about certain unclear but insistent organic complexes and muscular attitudes may help one to become familiar with them; even sometimes to control them. Talking about a thing which one fears may either increase or decrease the fear, but seldom leaves it unchanged. *Mutatis mutandis*, this is probably true for all instincts, especially those connected with sex. Finding or inventing words for these unclear experiences, using them with meaning, and, uttering them so often that they become speech habits, may dull the edge of fear, lust, and more complex emotions. How, exactly, verbalization performs this liberative function is not known.

Expressing frankly in words, to oneself or another, unclear experiences which have stirred one up organically, seems to have an early and a late effect. At first, talking about 'censored' but dawning experience might increase the intensity of its concomitant organic disturbances. Examples of such experiences are those connected with fear, sex, or even, in some persons, acquisitiveness. Such disturbances are produced in some people by reading Mr Bertrand Russell's *Sceptical Essays* or Dr J. B. S. Haldane's *Daedalus*.

At a later stage, familiarity breeds not contempt but automatism. Mr Michael Arlen causes less excitement as time goes on. And when one discovers some of Mr D. H. Lawrence's formulae, his writings are less perturbing, though never perhaps really restful.

A VISUALIZER'S ATTITUDE TOWARDS THE 'VERBALIZATION' (*sic*) OF HIS THOUGHTS.

Professor James Dewey has recently told us that the novelist and dramatist are much more illuminating as well as more interesting commentators upon conduct than the schematizing psychologists[1]. A contribution of these latter towards the dullness of life is the crude, question-begging supposition that visual imagery can be *expressed* in words, as pounds can be expressed in francs. Yet does anyone, not already besodden with words, either believe or attach any real meaning to it? In modern non-academic writings we find this expressed better than in a shelf of textbooks on psychology, with that brilliant exception *Studies in Aphasia* by Sir Henry Head.

Miss Victoria Sackville-West writes, in *Passenger to Teheran*:

> It may be that language, that distorted labyrinthine universe, was never designed to replace or even to complete the much simpler functions of the eye. We look; and there is the image in its entirety, three-dimensional, instantaneous. Language follows, a tortoise competing with the velocity of light, and after five pages of print succeeds in reproducing but a fraction of the registered vision. It reminds one of the Oriental who with engaging naïvety thought that by photographing the muezzin he would record also the notes of his call to prayer. The most—but what a most!—that language can hope to achieve is suggestion; for the art of words is not an exact science. We do not indeed reflect often enough how strange a world-within-the-world we have created by this habit of language, so strongly rooted in us by tradition and custom, so taken for granted, that we are no longer capable of imagining life without it, as one of those ideas which the mind is unable to conceive, like the end of time or the infinity of space....
>
> Moreover, the contradictions contained within the capacities of language are violent and astonishing. At one moment it seems that there is nothing (within the limits of our experience) that may not be expressed in words, down to the finest hair-stroke of a Proust or a Henry James; next moment we recognize in despair, so poor is our self-imposed vehicle, our incapacity truly to communicate to one another the simplest experience of our factual or emotional life. Who amongst us could boast that, transplanted into the mind of another person, even though that person be his nearest, he would not find himself in a strange country, recognizing here and there a feature that he knew, but on the whole baffled by unexpected grouping, shape, and proportion? There is only one province of life with which language is almost fitted to deal; the province of the intellect, because that is the province, so to speak, begotten by language itself, which without language would never, could never, have come into existence. Those things which are felt, and those things which are seen, because they exist independently, and in no ratio to the degree of our articulateness, are not the business of words.

Mr Robert Graves in *The Meaning of Dreams* and *Poetic Unreason*, and Mr J. D. Beresford in the first pages of *Writing Aloud*, explicitly deny that in any way words can be the equivalent of the experiences accompanying visual imagery.

So, feeling that to surrender his intimate experiences for the worn counters of common exchange is to distort and cheapen them, the visualizer is unenthusiastic about the revelation to others of his private techniques, even if this be possible; which, I imagine, some cartoonists and caricaturists would doubt strenuously.

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[1] *Human Nature and Conduct*, London, p. 154.

I do not suggest that the verbalizer has no mental privacy[1]; that his meanings are entirely expressed by his words, that, so to speak, his whole mental life takes place on the front porch. Yet, according to some behaviourists, the verbalizer's processes are, in theory at least, open for all to observe; the intellectual, as Mr Bertrand Russell remarks, being merely an acrobat with his linguistic mechanisms.

A number of psychological problems lie rather inaccessibly in the fissure between visual imagery and its so-called verbal equivalents. At present, workers on both sides of the crevasse seem too busy to notice it, or its contents. Yet the fact that daily leaps are made across it by psychologists—to say nothing of writers upon language—must be recorded by any fair observer.

REALLY SUBTLE SKILLS.

My thesis will now, I hope, be plain; low-grade coarse skills fit fairly well into the behaviourist's programme of study; subtler ones do not. Yet a fair number of their problems can be squeezed in. For the main framework of the bodily expression, even of delicate adjustments involving social and forensic skill, may become habitual; *i.e.* an order of acquired specific responses. 'Skill' may usefully name the executive aspect of the performance. But skill also has a specifically psychological component; kinaesthetic knowledge. This is the awareness of the acts, either as they are carried out, or as they are not carried out, for inhibition is a positive occurrence. This awareness will present cognitive, affective and conative aspects, the relative aggressiveness of which will differ at different moments.

What is to be said of the skill in anticipating, by means of kinaesthetic sensations? So far as they function in this way they are the experiential counterpart of the externally exhibited behaviour. But anticipation by the use of non-kinaesthetic, including many non-verbal images, includes myriads of everyday occurrences which decline even to approach this explanatory bed of Procrustes. Some of them, especially the visual images, annoy simple-minded behaviourists as the *ignis fatuus* got on the nerves of early scientists. Ford Madox Brown has depicted John Dalton, prone above a meadow pool, collecting marsh gas in a jar. A puzzled and contemptuous cow watches him, deploring, maybe, that he cannot employ his time more practically. Yet by studying the flickering uncanny apparition it was brought inside the framework of chemistry. This was not done by imploring people to give up the idea that the will-of-the-wisp existed.

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[1] Cf. "Privileges and Limitations of Visual Imagery," this *Journal*, 1925, xv.

Similarly, though the visual image may be a slippery customer to deal with, we cannot leave it out of any proposed explanation of skill. Whether anybody would even propose to omit the auditory image from a consideration of skill in musical composition, I do not know. It may be that the relationship between the image and the executive behaviour which accompanies it is the counterpart of the relationship between that experienced fact called a percept and the behaviour which accompanies it.

Perhaps therefore we may call the acts of collection, discrimination, rejection, ordering; in short all those events described on p. 149, as the prelude to skill. They anticipate and adumbrate, as to the experienced hearer, the Prelude to *Tristan* touches upon the whole story. It may be that in the prelude to skill the essential can be usefully distinguished from the inessential preliminaries. When the skilled action is swift, the anticipations are interwoven with the total performance, and never separately articulated, as this spacing-out of the phrases describing them would suggest, though many skilled actions do definitely follow such a prelude.

I will merely mention certain important skills towards the partial analysis of which these distinctions may be of use.

What is mathematical skill?

I am of all men the most unfitted even to speculate concerning the psychological nature of mathematical skill. It demands however the capacity to see relations. This, when exercised in any particular direction for the first time, is called intelligence. It may be assumed that the frequent grasping of relations of a particular type may become a habit. Thus some mathematical skill might conceivably become kinaesthetic knowledge, low grades of which give mere facility in arranging symbols; as, for instance, factorizing an algebraic expression.

Skill in literary composition.

About skill in literary composition I will say little, but (unnecessarily) remind the reader that some books offer to teach the art of writing. Whether many of them do more than to lay down canons, as a book professing to teach tennis might vouchsafe merely a reprint of the rules, I do not know. Mr Beresford, in *Writing Aloud*, and other writers, have occasionally allowed us to see them at the work of composition. And it will please some and offend others if I suggest the complexity of the tasks which would confront a physiologist and psychologist if they tried to write down the problems offered by the fashionable or obsolescent skill of certain 'biographers': Lion Feuchtwanger, Emil Ludwig, André Maurois, Lytton Strachey and, in her latest phase, Virginia Woolf[1]. Everyone will regard them as highly skilful; a few as all-too-skilful.

ARE WE BECOMING MORE CONSCIOUS?

In an essay with this title[2], Mr I. A. Richards suggests that people, particularly some Americans, are becoming more clearly aware of aspects of themselves which until recently have formed an unanalysed background. Some Englishmen think some Americans shallowly enthusiastic about trifles, apt excitedly to emit platitudes about axioms, to blurt out things which 'decent' people would keep to themselves. Such Englishmen probably regard such American minds as having little background, their own extending almost to infinity. On the other hand, some Americans often think that the Englishman is silent because he has nothing to say. As to this English background, the American is not necessarily enthusiastic about it. He may deem it to be of the nature of undergrowth, and rather dank.

Mr Richards and some other writers suggest that we are becoming aware of more of our mental processes, as well as of more things in the outside world. This does not imply merely the uncovering of our minds, as bodies are uncovered, but becoming more clearly aware of certain aspects of our minds, as people have recently become more conscious of their bodies. By parity of reasoning, one may expect an increase in personal mental hygiene, in personal mental cosmetics, and the payment of undue attention to certain aspects of the mind.

The above describes generally a change in usage exemplified by the increasing tendency to describe, analyse and share techniques and skill. The words cunning and craft, long associated with skill, nowadays suggest to many only their secondary, less exalted meanings. It is often felt that a person who keeps skill 'up his sleeve' shows craft in both senses of the word. More significant is the modern attempt to analyse, and thus to destroy as a craft, all craft-skills. That this movement is resented and resisted by the cunning and crafty workers is easy to understand.

[1] Cf. *Orlando*, London, 1928.

[2] *Psyche*, July 1927, VIII, 26–34.

The impression seems justified that the number of books on 'How to do' this, that and the other, has greatly increased of late. Nowadays it is distinctly fashionable to write about technique. This fashion is worth discussion.

That some of these books are excellent and others pitiful is obvious. Yet the ignominious failure of one person to describe a technique does not prove that the feat is impossible. Many techniques can be adequately described[1]. It is possible to believe that of no valuable technique can the whole story be told. Even here, however, one should make sure that the wish is not father to the thought.

SKILL, TECHNIQUE AND DECENCY.

There is a tendency at present to publish techniques which used to be 'decently' reserved or hidden. Even the discussion of intimate personal relationships is more open. Two recent books, *The Technique of the Love Affair* and the *Art of Making a Perfect Husband*, may be meant only half-seriously. This fact matters little except for readers who respect only dismal writing. Many journals, including a dignified English weekly, answer questions concerning the technique of social relations, and many books here and on the Continent deal with questions of courtship and marriage which until lately were seldom mentioned frankly.

This general interest in technique and skill has suggested the analysability of certain performances which used to be considered unique and incommunicable. Modern language teaching enlists experimental phonetics, with the help of the gramophone and the microphone. Even our sacrosanct English dialects, sedulously encouraged and warmly defended by persons who have spent small fortunes in eliminating their own, are being undermined. Analysis of speech is facilitated, so that it can be consciously altered[2].

A most interesting example of skill-analysis is Dr M. Metfessel's *Phonophotography in Folk Music; American Negro Songs in New Notation*[3]. He writes of "the curious turns and twists and quavers and the intentional striking of certain notes just a shade off the key, with which the negro loves to embellish his songs...an indescribable...subtle effect which only the true 'jubilee voice' can produce...it consists in certain turns, twists...not represented by any musical term." He claims that his phonophotographic technique disposes of the assumption that music and speech cannot be 'notated.'

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[1] Examples cited in *Fitness for Work*, London, 1928, Chapter II; and *Skill in Work and Play*, London.

[2] Cf. Mr A. Lloyd James's broadcast course for schools, *Speech and Language*, British Broadcasting Corporation pamphlets, 1928.

[3] With introduction by C. E. Seashore, 1928, University of North Carolina Press, and Oxford University Press.

That this restless tendency to analyse all skills, and, consequently to experiment with new ones, may lead to some failures, is to be expected. But they would not destroy the truth of the above assertions.

To many, the uncovering of a private technique savours of the indecent. At its mildest, it is like wantonly driving an expensively furnished car with the bonnet lifted. We expect art to be concealed. Grock, when he fails to play his little fiddle while throwing up his bow, retires behind a screen to practise. This we feel to be only proper. And how whole-heartedly we laugh at a conjurer, who, letting us into the secret of a trick, gets it wrong!

Resentment seems most intense where the personal touch is analysed. We smile at the discovery of any deliberate attempt (when it fails) to alter personality. Some years ago, a touring team of debaters met to discuss possible improvement in their methods. Inspired by one university, whose undergraduates had debated casually, nonchalantly and wittily, the team resolved to learn up some more jokes. Why does this amuse us?

Turning to another aspect of the question, it seems probable that since respectable people show to the world more of their bodily and mental selves than was customary, ideas of decency are changing. It has become permissible or even *chic* to run one's personality with little more than its engine and chassis, and the minimum of upholstery.

Tolerance of the public alteration by women of their facial appearance; objection by some persons to certain aspects of this toilet, and by others to other aspects, these facts show the way the wind is blowing. How far this greater openness is a revolt against Victorian customs and to what extent it is a conscious means of attracting attention is an interesting question.

Another fashionable form of this deliberate baring of personality is quasi-cynical reference to one's motives, when announcing decisions. Often, however, this is a subtle defence, for by lightly attacking one's own weak points, an enemy's subsequent thrusts at the same places are deprived of their sting.

While studying the expression of personality by means of the voice[1], I was led to suspect that some would think it indecent to analyse the type of voice acquired by the English ruling classes. Probably it can be analysed. It should not prove more recalcitrant than a negro dialect. But many would feel discomfort at the prospect.

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[1] "Voice and Personality," *Radio Times*, 14 Jan. 1927 (full report unpublished).

THE STYLIZATION OF SUBTLE SKILLS.

It is interesting to note the extent to which skills, becoming 'stylized,' diverge from their original aims. One can gauge the angle of deviation by noting the contempt shown by the practitioner of the skill, in what he holds to be its 'natural' form, for the new upstart 'artificial' style. The speed-skater's attitude towards the figure skater, the *Langlauf* ski-runner's coldness towards 'downhill only' acrobatics; the older French and Italian ballet-dancers' view of the Russians' presentation of *L'Après-Midi d'un Faune* or *The Cat*, are simple examples. This 'what-are-we-coming-to' attitude is not unknown in critics of literature, art and music.

THE FATE OF SKILLS THAT ARE LEFT ALONE.

The present tendency is to examine, analyse, discuss, ventilate and —to borrow an expression from America—'de-bunk' even the most sacred skills. Those not subjected to such treatments may become stylized, causing pride in some and annoyance in others, or stagnant. Stagnation may be deliberately continued, becoming a special style. Ceremonial skills in the church, the army, and the universities exemplify this.

Often stagnation continues to exist in spite of continual demands to end it. Mr William Clissold believes this to be true of the teaching in the provincial universities[1]. (This phrase, the context suggests, describes all English universities, outside London.)

The present article is unsuited for the opening of a hornet's nest. But it seems certain that where there is no effective criticism, as distinguished from the speech habits called 'grousing,' skills will stagnate, even putrefy.

And university teaching notoriously becomes stylized. An unpleasant modern habit is contemptuously throwing to a class shreds of ill-prepared information, with the suggestion, often true, that the 'teacher' is wasting time which would have been more profitably spent in research.

A skill, which, left alone too long, is said to have degenerated, is preaching. There are probably many reasons why it has been left alone, but from time to time it is examined. One of its aspects has lately been discussed at length, by preachers, in a daily newspaper[2].

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[1] *The World of William Clissold*, Book 5, pp. 733 *et seq.*

[2] "The Preacher's Voice," letters in *Manchester Guardian* of 8th Jan. 1929, and subsequent days.

It is doubly significant that the British Broadcasting Corporation is continually asking, and usually in vain, that the newspapers shall review its productions.

PREVALENT ATTITUDES TOWARDS IMPORTANT SKILLS.

Towards important skills, society adopts a graded order of attitudes. Two of these are sharply contrasted. The first encourages persons to harden or stereotype a technique, and to encrust or encapsule it in a mass of ancillary rites, ceremonies, traditions, customs and habits. These secondary techniques in their turn often demand special exponents, to whom the core inside these wrappings and trappings may be inaccessible. Minor priests, footmen, college porters, and some sergeant-majors are examples. They usually develop for their profession sentiments of immense power. These would lead to conservatism even in progressive minds.

The army, navy, and universities are particularly given to skill-mummification. Their resistance to its public unwrapping is easily understood. Mr Robert Graves in *Lars Porsena*, that review—or revue—of a linguistic skill, suggests that though it may be difficult to explain to the next generation what blasphemy meant in our time, criticism of the navy will still be regarded as an obvious example.

The contrasted tendency in our own country, but more especially in less conservative ones, and in those where revolution has followed conservatism, is to take down and rebuild skills of all kinds. That this attack is resented by hieratically-minded practitioners is interesting but comprehensible. Yet not all their pentagrams strewn in his path will deter the modern analyst of movement.

These opponent attitudes can be illuminated in one example. In England there is now a tragic twilight of the old-style music-hall stars. This is attributed to their adherence to outworn techniques and to their younger rivals' incessant experimenting with new ones.

THE SOCIAL MEANING OF THESE CONSIDERATIONS.

This article, beginning as a discussion of skill, ends like an introduction to a sociological discussion. For not less important than the microscopic examination of skill is the macroscopic observation of its place in the life of the people. While in certain respects the first examination seems to be progressing healthily, the second task is less often undertaken.

In conclusion, some social considerations will be indicated.

(1) It is wise occasionally to overhaul our proudly practised techniques. If we do not, others with a different mental outlook will do so. To become sentimentally attached to any skill, except for the sake of ceremonial—an excellent thing in its place—is unwise. Possibly, too, we should realize the danger of becoming victims to an 'obsession to repeat.'

(2) While recording or deploring the subsidence of certain skills it is only right to insist that (*a*) entirely new important skills have recently appeared; *e.g.* many connected with practising, and supplying the mechanical means for, aviation; (*b*) some traditional skills are re-vivified, *e.g.* swimming has been scientifically improved.

(3) Increasingly successful attempts are being made, by example, by precept and by visual demonstration, to share the subtler skills with others. The increasing intellectualization of skill, through rapidly improving technique in photography, cinematography, and in using descriptive words for writing, speech, and broadcasting will popularize widely many techniques. How far such analysis will dissipate the 'personal touch' is at present disputed. Here, the possibility of an unconscious motive in the skill-exponents who discourage analysis cannot be discounted.

(4) The protest, or snarl, at the 'invasion of personality' which even the hint of skill-analysis evokes, is quite healthy. By some innovators in this region it has been ignored or misunderstood. Yet the grounds of the protest are not always clearly stated. Liberty to express one's personality through a skill should be distinguished from licence to muddle through (sometimes miscalled initiative) or to develop consistent but clumsy and wasteful methods of dealing with expensive material.

(5) Attacks upon established skills are useful when they are not mere fulminations but demonstrate the limitations of the skill. For example, criticism of the pianist's skill, with his notes of fixed pitch, by lovers of stringed instruments may thus be profitable.

(6) The separation of visual and auditory knowledge from kinaesthetic knowledge will certainly increase. Understanding the significance of this development is indispensable for the industrial psychologist.

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THE RELATIONSHIP OF SUBJECTIVE EXPERIENCE TO THE PERFORMANCE OF EYE MOVEMENTS

By M. D. VERNON.

(*From the Cambridge Psychological Laboratory.*)

1. *Introduction* (pp. 161).
2. *Description of experiments* (pp. 161–163).
3. *Kinaesthetic sensations and accuracy of movement* (pp. 163–164).
4. *Kinaesthetic sensations and fixation pauses* (pp. 164–165).
5. *Awareness of meaning and interest* (pp. 165–167).
6. *Awareness of perceptual processes* (pp. 167–168).
7. *Individual differences in performance and introspection* (pp. 168–170).
8. *Conclusion and summary* (pp. 170–172).

1. INTRODUCTION.

IT is well known that in general we are unaware of the exact nature of the movements of our eyes from side to side of the visual field. In reading, the eyes seem to move smoothly along the printed lines, and not in a series of jerks alternating with pauses of greater or less duration. Moreover, since movement is very rapid, it is probable that blurring of the image would occur did not central anaesthesia take place meanwhile. Yet if we glance across the visual field and perceive an object placed at its centre, we are usually unconscious of any fixation pause on that object, and consider that a glimpse was obtained of it in passing. Therefore, in a series of experiments recently performed upon the direction of eye movements and the position of the subsequent fixation points, it seemed of some interest to determine what relationship, if any, existed between the objective performance and the subjective experience which accompanied it.

2. DESCRIPTION OF THE EXPERIMENTS.

The object of the experiments was to discover the accuracy with which the eyes could move from the right to a point on the left, after the successive fixation, from left to right, of a series of stimuli. These stimuli, arranged in pairs of horizontal lines, consisted of two or four block capital letters per line, or five disconnected words, or five words connected together in such a way that the ten words of the pair of lines formed a complete sentence. The letters and words were black, and the field white. The subject was first required to fixate a black cross on a white screen placed in front of the test material; the cross occupied the same position as the first word of the second line. The screen then dropped, exposing the lines of letters or words, and the subject read them, fixating each letter or word separately. By means of a suitable reflecting apparatus, the path of the eye movements could be followed, and the position of the eye at the preliminary fixation compared with its position when fixating the first letter or word of the second line, after the return movement from the end of the first line. Thus the accuracy of return to the beginning of the second line could be determined with some certainty.

With some of this material were presented isolated letters, words, or small drawings (in black) lying between the lines, either directly in the path of the return movement, or just above or below it. Half the drawings were conventional diagrams, and the other half pictures of real objects. Letters and drawings were presented with both the letter series, and words and drawings with the disconnected word and sentence series. In the latter, the pictures were of gradually increasing complexity. It was possible to observe whether the return movements were interrupted by fixation pauses upon these visual obstructions. The subjects were instructed not to fixate anything besides the letters and words of the two horizontal lines.

In order to throw some light on the nature of the tendencies at work in this performance, a fair amount of introspective evidence was collected. It should be noted that all this introspection was not free. A considerable number of questions were asked, such as: "Did you have any difficulty in making the necessary movements? Did you notice the middle letter at all? Did it cause any distraction?" Now these questions are all of the 'subjective direction' type, described by Muscio[1] in an investigation into the influence upon the answer of the form of a question. The accentuation was on the subjective experience; and the answer was more frequently in the affirmative than in the negative. Muscio showed that the reliability of answers to questions of this type was very high. Also the answers to the questions in this investigation varied considerably from time to time, *i.e.* no persistent suggestive influence appeared to be at work. Nor did the experimenter have any preconceived theory as to the results obtained, which would be likely to influence the questions and increase their power of suggestion in any particular direction. Moreover, a good deal of additional elaboration was volunteered which usually rendered it possible to obtain a fair idea of the forces and tendencies at work in the mind of the subject; and it is from this that the following conclusions have been drawn.

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[1] Muscio, B., this *Journal*, 1908, VII.

The experiments were performed by ten subjects, all students of psychology accustomed to introspect, and probably more than usually resistent to suggestion.

3. KINAESTHETIC SENSATIONS AND ACCURACY OF MOVEMENT.

It might be thought that the subject would be completely unaware of the nature of his performance, and that his subjective experience would not correspond in any way with the efficiency and correctness of the movements performed. No estimate of the accuracy of the return movements was either requested or volunteered. But the subjects were asked at some time during each experiment to say if they experienced any difficulty in making the required movements, and to give any reasons for that difficulty if it was felt. Five of the subjects found the letters which were arranged two in a line more easy to read than those arranged four in a line. The letters of the latter series were thought to be so near together that the movements were unduly short (actually each covered more than 3° of arc); it was difficult to pause in the right position, and not to go beyond the correct point. It is probable that this was manifested, at least at first, in hurried, jerky, irregular movements from one letter to the next; no observations were, however, made to determine this. But it is interesting to note that the inaccuracy of the return movements of these subjects was on the average nearly 40 per cent. greater in reading the four-letter lines than in reading the two-letter lines. It is possible that adjustments necessary for the short forward movements and for the long return movements were mutually conflicting and incompatible; hence the judgment that the forward movements were too short, and the inaccuracy of the return movements. Two subjects, however, found the letters arranged two in a line more difficult to read. One of them felt a tendency to drop straight to the second line of letters instead of moving horizontally across. The other found it difficult to move right across the field in one unbroken movement. These tendencies were generally resisted, but they seemed to indicate a difficulty in making long accurate movements, for the return movements were on the average over 40 per cent. less accurate in this series than in the four-letter line series.

In reading the disconnected word series, the return movements of six of the subjects were 40 per cent. *less* accurate than in reading the letter series; but those of four of the subjects were 15 per cent. *more* accurate in reading words than letters. The former subjects reported that the disconnected words were more difficult to read than the letters; the motor habits acquired in the letter series apparently were not utilized, for the movements were felt to be slower and more jerky and irregular. It seems that there was some difficulty in treating the words as isolated stimuli, partly because they were less legible and covered a larger spatial area, and partly because there was an unsuccessful attempt to combine them into the familiar sentence pattern. In the sentence series, the subjects experienced a sensation of ease, as though they were carried on smoothly from one word to the next by means of the connections between the words. In consequence, the accuracy of the return movements of these subjects was 70 per cent. greater in this series than in the disconnected word series. It seems that the habitual easy to and fro movements which have been developed in normal reading were utilized in this series, while they could not be employed in the disconnected word series.

The other four subjects, however, were much less variable, both in objective performance and in subjective experience. In general they reacted to the words in much the same way as to the letters—that is to say, they treated them as isolated stimuli to be fixated separately. The average inaccuracy of their return movements was 15 per cent. *less* in the disconnected word series than in the letter series; but this difference was probably not significant. In the sentence series, deeply-rooted normal reading habits seem to have been inoperative, for the inaccuracy in this series averaged 25 per cent. *more* than in the word series. The tendency appears to have been to separate the words in this series also; and the effort to fixate all the words, according to the instructions given, broke down the normal reading habits.

4. KINAESTHETIC SENSATIONS AND FIXATION PAUSES.

If we now study the interruption of the return movements by the fixations of the visual obstructions lying between the lines of the text, we discover at first little parallelism between the actual number of pauses in the return movements and the objective performance of the subjects. The latter constantly over-estimated their ability to inhibit fixation of the visual obstructions during the return movement. They were in general aware of fixation pauses of considerable duration, when the obstruction was studied in some detail. These pauses may have been involuntary in their inception, but they were to some extent continued voluntarily. Involuntary pauses of very short duration were not usually cognized; the subject considered either that he had perceived the object peripherally while fixating the letters or words of the text, or else that he had glanced at it in passing without stopping to fixate it. A real difference appears to have existed between fixation pauses described as 'looking at' something, and 'glancing at' it during movement. It is not possible from these observations to assign definite time limits to either type, but it seems clear that both the kinaesthetic sensations and the visual perceptions experienced were qualitatively different in the two cases. The eyes were judged to be moving throughout the short pauses, but to remain at rest during the long pauses. In the latter the visual obstructions were examined in detail, their full meaning and significance determined, and a position assigned to them in the complex association patterns of previous experience. But during the unconscious fixation pauses, perception was vague, so much so that it was often thought to be peripheral rather than foveal. Details were omitted or altered, and sometimes a wrong meaning or name was assigned. It is probable that these short fixation pauses were of the type normally employed in that vague perception of our surroundings which only enters the fringe of consciousness, to form the background or setting for the objects which we scrutinize and attend to in detail.

The forward movements made in reading the top line of letters or words were also much affected by the visual obstructions. It was rather difficult to distinguish clearly the abnormal hesitations and regressions from normal fixation pauses on the letters or words, unless the former were prolonged. However, it appeared that on the whole it was only the very long hesitations and regressions of which the subjects were aware. But regressions caused by inability to read the text, whether as the result of illegibility of the words, or of confusion of the meaning by the obstructions, were usually cognized; and so also were some of the hesitations which occurred early in the experiments as a result of unfamiliarity with the required movements. Otherwise, the subjects generally considered that they did not fixate the obstructions during the forward movements, unless the pauses were very prolonged, but perceived them peripherally.

5. AWARENESS OF MEANING AND INTEREST.

If the introspective data so far described appear to bear little relationship to the actual processes of movement and fixation, it must be remembered that, after the early stages of the experiments, kinaesthetic sensations were rarely at the focus of consciousness. But an important class of tendencies, connected with the meaning and interest of the text and obstructions, were much more completely cognized. The subjects were in general fully aware of their perceptions of the material presented, of the interest aroused, and of the connections and conflicts between the organized arrangements of meaning which constitute the patterns of language and thought. The relation between these tendencies and the objective performance was not always clear at first; but upon further consideration, the effects of these tendencies upon motor behaviour proved to be of great importance, particularly with certain subjects. The following principal facts emerged:

(1) More than twice as many pauses were made on word obstructions in the disconnected word series than on word obstructions in the sentence series. Yet these obstructions were thought to interfere far more with the reading of the sentences than of the disconnected words.

(2) The drawings of real objects were considered to be intrinsically interesting and attractive, but not to interrupt and upset the reading of the sentences so frequently as had the word obstructions. In this series there were slightly fewer pauses in the return movements on the pictures than on the words. But the pictures were reported to cause some distraction in the letter and word series, where they were responsible for 11 per cent. more pauses in the return movements, on the average, than were the word and letter obstructions (in the central position, directly in the path of the return movement).

It was pointed out above that the sensation of ease and smoothness of movement reported by most subjects denoted the existence of strong motor habits in reading connected sentence material, as shown by the accurate adjustment of the eye movements in the sentence series. It seems reasonable to suppose that these motor habits were also responsible in part for the comparative freedom from interruption of the return movements in this series. But these were not the only habits of importance. The sentence forms the accustomed unit of speech, and, probably, also of thought, at least of verbal thought; and to many individuals the isolated word is not a separate unit, but part of a potential phrase or sentence. Hence the tendency constantly at work to connect isolated words into some group or organization, and the feeling of interest or satisfaction when this was possible; hence also the conflict experienced when the word obstructions could not be fitted into the sentence pattern with which they were presented. In the last case, well-formed motor habits continued to guide and direct the eye movements; but the organization and arrangement of the meanings of the words was prevented, with the consequent feeling of distraction. In the disconnected word series, the obstructing words and the words of the text were all of equal value, either as isolated units or as parts of a potential sentence whole. There was a strong tendency to fixate the obstructing words in order to find out their meaning, but there was no struggle between opposing meanings. In this series the chief difficulty, at least for some of the subjects, was a motor one, resulting from the mutual inhibition of the motor habits appropriate to the successive fixation of isolated stimuli, and the deeply-rooted habits of normal reading. This increased inaccuracy more than it increased interruption of the return movements.

While the subjects regarded the pictures of real objects as isolated units, they found that the latter did not conflict with the reading of the sentences, although they were intrinsically more interesting. That is to say, the pictures did not interfere with the connected sentence pattern because they belonged to a different mental category, and the subjects felt no tendency to bring them into the sentence pattern. But if they either attempted to establish a connection or relationship between the pictures and the sentences with which they appeared, or allowed their attention to centre upon the relationships of the pictures to each other, a struggle was experienced, with the frequent result that all meaning and comprehension of the sentence content was lost. The accuracy and continuity of the eye movements were still remarkable; clearly the necessary movements could be executed effectively under very considerable distraction. For the disconnected word series presented a different case. Here there was no conflict. The intrinsic interest of the pictures was alone sufficient to fill the attention and impair greatly the accuracy and regularity of the eye movements. It is interesting to note, however, that the number of pauses on the pictures in the return movements was greater in the disconnected word series, where there was a connection between the successive pictures, than in the letter series, where there was none; and this result agrees with the comparative interest in the two sets of pictures which was reported by the subjects.

6. AWARENESS OF PERCEPTUAL PROCESSES.

The conventional diagrams aroused little interest and did not cause nearly as much distraction as the pictures of real objects. They were thought to interfere considerably with the reading of the letters and to some extent of the disconnected words; but not much with the sentences. Actually the number of pauses on diagrams was large in both disconnected word and letter series, sometimes greater and sometimes less than the number of pauses on the pictures of real objects. The number was small in the sentence series, but considerably greater than the number of pauses on pictures of real objects.

If the reasons given for the effect of the diagrams be now considered, three factors of importance appear, all of which were related to the perceptual processes. The first was the visibility of the obstruction, its distinctness from its surroundings. The diagrams were rather larger and blacker than many of the other visual obstructions; moreover, they stood out from the words and letters of the text by reason of their contrasted form. They exerted a kind of involuntary attraction on the eyes, as appears from the reports of 'something there,' 'something to be skipped.' The second factor was the structural simplicity or complexity of the object. The diagrams were almost all structurally simple, as compared with the pictures of real objects. But more important than this was the third factor, the ease or difficulty with which they could be apprehended. None but the simplest diagrams, a cross and a square, were familiar; they could not be named or fitted into some previously formed concept or organization of mental material. These two factors were in opposition. Some subjects reported that fixation of the diagram obstructions could not be avoided, because the latter were ambiguous, and could not be apprehended without long scrutiny. Other subjects could perceive them clearly, even in peripheral vision, on account of their structural simplicity; and thus felt little tendency to pause on them. But since they possessed no meaning and little interest, they generally disturbed only the motor processes, and rarely the thought or language processes.

The picture obstructions generally possessed good visibility, with the exception of those in the sentence series, and were readily apprehended; but they were structurally complex. The visibility and the structural complexity both strengthened the tendency to fixate these obstructions; for it was impossible not to see that they were there, but not always easy to see what they were.

7. INDIVIDUAL DIFFERENCES IN PERFORMANCE AND INTROSPECTION.

It is interesting to compare the various objective results and introspective record from the point of view of individual differences. Four of the subjects showed considerable inaccuracy throughout, and made a large number of pauses in the return movements. The inaccuracy and the number of pauses were both greatest in the disconnected word series and least in the sentence series. These subjects experienced considerable difficulty in reading disconnected words; they found it harder to form regular motor habits in reading this type of material than in reading the letters, since the habits appropriate to the successive fixation of isolated stimuli were destroyed, and normal reading habits could not be utilized. Moreover, the tendency to fixate the word and picture obstructions was very strong. In reading the sentences, a kinaesthetic sensation of ease and smoothness was experienced; but a feeling of conflict was aroused by the inability to fit the obstructing words into the meaning pattern. The pictures caused comparatively little distraction until it was realized that they too possessed an organized relationship; interest in this relationship then completely superseded apprehension of the sentence content. It was concluded that these individuals on the whole adopted a subjective attitude towards the experiments. They were concerned with meaning, significance and interest rather than with objective accuracy of movement; consequently they had some difficulty in forming motor habits appropriate to the fixation of isolated stimuli, but placed great reliance upon the well-formed habits of normal reading.

Three other subjects presented an almost complete contrast. They were accurate throughout, and made few pauses in the return movements. Moreover, the inaccuracy and the pauses occurred with comparative regularity throughout, though the former was slightly greater in the sentence than in the other series. It appeared that these subjects reacted much more objectively to the experimental conditions; that is, they concentrated upon the effort to perform the required movements regularly and accurately. Consequently they were able to develop habits of movement from one point to the next which were well adjusted both for the letters and for the disconnected words. These, rather than normal reading habits, were utilized in the sentence series; for the subjects reported that, with some difficulty, they read the words as isolated units, and not as parts of the sentence whole. Moreover, these motor habits precluded the interruption of the return movements, even by obstructions of considerable interest. The peculiar conflicts between meaning organizations were infrequent in occurrence, presumably because more attention was paid to the motor processes, and less to meaning and significance.

The remaining subjects presented somewhat anomalous cases. Two of them adopted the objective attitude as a rule, and paid much attention to regularity and accuracy of movement. But they possessed poor motor control, and though they made few pauses in the return movements, the inaccuracy of the latter was considerable. But apparently motor control was fairly adequate when the deeply-rooted habits of normal reading were operative, as in the sentence series, and to some extent with one subject in the disconnected word series.

The last subject adopted a very markedly subjective attitude to most of the experimental material. He was particularly interested in constructing connections and relationships between the words and pictures. But his accuracy of movement was fairly high, and he made very few pauses in the return movements. Accuracy appeared to be approximately the same in the letter, word and sentence series, presumably because these interested him equally (he reported considerable interest in both the letters and disconnected words). But it differed considerably for the different visual obstructions, because the intrinsic interest of these varied greatly, from the diagrams, which he considered completely uninteresting, to the connected pictures, which he related most ingeniously to the words and sentences with which they appeared. His performance also varied from day to day, according to various moods and affective states to which he was liable. But it appeared from his introspections that he also paid much attention to his objective performance, and made considerable efforts, in particular, to avoid fixating the obstructions during the return movements. Moreover, he possessed excellent motor control of the eyes. It seems that the introspective evidence which indicated a subjective attitude towards the experimental material did not altogether conflict with the objective results. But this individual probably exerted an unusually large amount of conative effort; hence he was able to carry on simultaneously the pursuit of accuracy and of meaning, while the other subjects, in whom conation was less powerful, found these pursuits mutually exclusive.

8. Conclusion and summary.

In conclusion, it appears that the subjects were unaware of the exact nature of their eye movements. They could only report the fixation pauses of greater duration, and the more considerable derangements of the movements, *i.e.* by long regressions or hesitations. These reports were not reliable, however; and rapid regressions and short hesitations went unnoticed. But, on the other hand, kinaesthetic sensations of the general ease and regularity of movement were closely related to objective performance, particularly in comparing the various results of the same subject. This was especially noticeable in the correspondence of ease and accuracy of movement in the letter, disconnected word and sentence series. It is not easy to account for the fact that certain subjects found the longer movements of the two-letter line series more difficult, and others the shorter movements of the four-letter line series; but reasons have been advanced for the inaccuracy of movement in the disconnected word series, and the accuracy in the sentence series. Comparatively little attention was paid to these kinaesthetic sensations. The individuals characterized by a subjective attitude were concerned with the content of the experimental material; while those who adopted the objective attitude thought more of *what* they did than of *how* they did it.

Hence much fuller and probably more reliable data were obtained as to the effects of meaning and interest; and as to the degree to which the mind was occupied by the respective meanings and interests of the text and obstructions, and the rivalries between the two. It was pointed out that much light was thrown upon the various anomalies in the objective behaviour, particularly with regard to the number of fixation pauses in the forward and return movements, by a consideration of these competing tendencies, and also of three factors appertaining neither to cognition nor to kinaesthesis, but to the actual perceptual processes. Unless all these factors are taken into consideration, it would be impossible to account for the relative effects of letter, word, diagram and picture obstructions. It was shown, also, how the relative results of the different subjects were explained to a great extent by a study of the tendencies and attitudes appearing from introspection. No complete conception of their motor behaviour could be arrived at without taking into account other unconscious tendencies, such as general motor control of the eyes and liability to impairment of accuracy with fatigue. We are, however, justified in advancing the data obtained from subjective experience as bearing a close relationship to the objective performance, and as elucidating to no small extent the reasons for the varieties in that performance.

To sum up:

A number of experiments were carried out upon the accuracy of the performance of a series of eye movements from right to left of the visual field, and the frequency of their interruption by fixation pauses on a variety of visual obstructions placed in their path. Introspective data were obtained, and their relationship to the objective performance considered.

Kinaesthetic sensations of ease or difficulty in making the required movements were closely connected with accuracy and inaccuracy of performance. Accuracy was greatest when the deeply-rooted motor habits of normal reading were operative; and was least when these were inhibited by habits acquired temporarily in the successive fixation of isolated stimuli.

The subjects were usually unaware of the shorter fixation pauses during which perception and apprehension were partial and vague; but they reported the longer pauses which were accompanied by close scrutiny and complete apperception of the visual object.

Introspective evidence revealed the importance, particularly to certain subjects, of the meaning and interest of the text and visual obstructions, and especially of the conflicts and mutual inhibitions of these meanings and interests. Perceptual factors connected with the visibility, structural simplicity and ease of apprehension of the visual obstructions also affected the objective performance.

Certain of the subjects, who were characterized by a subjective attitude towards the experiments, were more influenced by the above tendencies than others who adopted a comparatively objective attitude; and the performance of the former was more inaccurate and variable than that of the latter.

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EXPERIMENTS ON THE INFLUENCE OF THE AMOUNT OF REWARD ON LEARNING IN YOUNG CHICKENS

By G. C. GRINDLEY.

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1. *Introductory* (p. 173).
2. *Exp.* 1*: Food-tray in sight* (pp. 173–175).
3. *Exp.* 2*: Food-tray out of sight* (pp. 176–177).
4. *Exp.* 3*: Learning with no reward* (pp. 178–180).
5. *Conclusions* (p. 180).

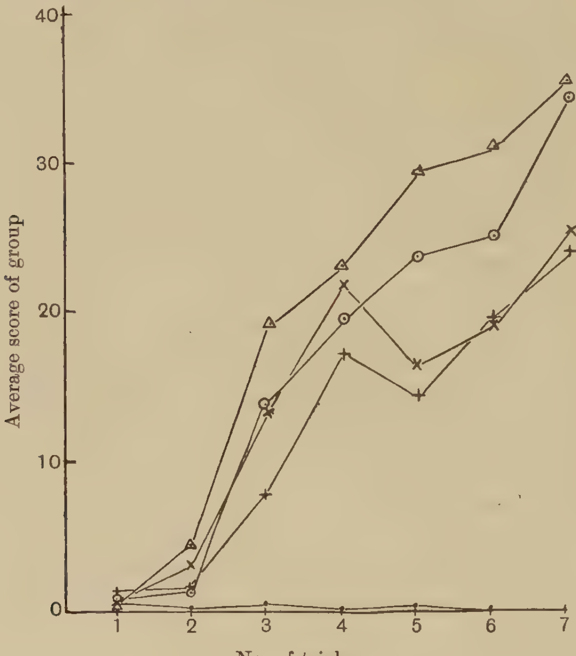
1. INTRODUCTORY.

IN many experiments on learning in animals the food-seeking tendency is used as an incentive, and it has been shown (1) that in these cases the rate at which an animal learns is dependent on its state of hunger. It might be expected that the rate of learning would also be dependent on the amount of food given as a reward for successful performance, but this problem, although it is of considerable theoretical importance, does not seem to have been investigated. Yerkes and Dodson (2) have, however, described some very interesting experiments on the influence of the amount of punishment given for unsuccessful performance on the rate of learning in mice, and Cole (3) has studied the same problem in chickens; while Hoge and Stocking (4) and Dodson (1) have compared the effectiveness of certain rewards with that of certain punishments, and Stone and Sturman-Humble (5) have compared the value as incentives of food-seeking and sex.

The present paper describes briefly some experiments on the influence of the amount of food given as a reward on the rate at which young chickens learn certain relatively simple performances.

2. EXP. 1: FOOD-TRAY IN SIGHT.

The apparatus in this experiment consisted of a passage 4 ft. long and 8 in. wide at one end of which was a release-box. In the preliminary training the chicks were trained individually to run out of the release-box immediately it was opened and to look for food anywhere in the passage. Seventy chicks were trained in this way from an age of five days to an age of ten days, each chick being given fifteen trials. Each chick was then tested by measuring the times taken by it in four trials to find a grain of rice placed at a fixed spot near the middle of the passage. From the results of this test the fifty 'best' chicks were selected, and they were divided into five groups of ten.



Curves 1.

• Group 0. + Group 1. × Group 2. ⊙ Group 4. ◬ Group 6.

In the actual experiments a small wooden tray was placed at the end of the passage remote from the release-box, and a certain quantity of food was placed on this tray; the amount of food being the same for each chick of a given group, but differing from group to group. A projecting ridge on top of the tray hid the food from the chick until it was within about 4 in. of the tray. The rewards given to the chicks of the various groups were as follows: Group 1, one grain of boiled rice; Group 2, two grains; Group 4, four grains; Group 6, six grains; and Group 0, no reward.

Learning curves were obtained in the usual way, each chick being tested only once in each day. All the chicks were kept together between experiments, and when they had been tested they were given a large meal of rice. At other feeding times they had no rice. In this way it was arranged that the chicks of the various groups should have as nearly as possible the same appetite for rice at the tests. The experiments were always made at a time when the chicks were hungry.

A difficulty which is often encountered in experiments on individual chicks is that of 'loneliness.' To obviate this four chicks (which, of course, were not taken from the fifty used in the experiments) were tethered with string at one side of the passage. It was found that after the first few days of the preliminary training the tethered chicks remained quiescent, and that the chicks taking part in the experiment took little notice of them.

In each test a chick was given a 'score' equal to one hundred times the reciprocal of the time in seconds taken by it to reach the food (or, in the case of the chicks of Group 0, the time taken to reach the tray) after the opening of the release-box. Thus a chick which pecked at the food after five seconds was given a score of 20. If a chick did not find the food in 120 seconds it was gently pushed along to it by the experimenter, and given a score of zero. Some of the chicks were found to develop a dislike for rice during the experiments. Any chick which failed on more than one occasion to eat the whole amount on the tray was disqualified, and all its scores were neglected. In all, thirteen chicks were disqualified: four from Group 1, one from Group 2, four from Group 4, and four from Group 6. The experiment began on the twelfth day after hatching and was continued for seven days.

Results.

The average scores obtained by the chicks of each group on successive days are shown in Curves 1. It will be seen that, under the conditions of this experiment, there is a marked increase in the rate of learning with increase in the amount of the reward. If we take the total of the average scores of any group as a rough measure of the rate of learning of that group, we find that this rate is about 60 per cent. higher for the chicks of Group 6 than for those of Group 1.

Towards the end of the experiments it was noticed that most of the chicks of Groups 4 and 6 walked or ran briskly down the passage, showing some signs of excitement, while the chicks of Groups 1 and 2 usually proceeded in a more leisurely way. But there were wide individual differences in behaviour.

3. EXP. 2: FOOD-TRAY OUT OF SIGHT.

It was thought that the increase in the rate of learning with the amount of the reward in the experiments just described might be due, wholly or in part, to the fact that in those experiments the food-tray could be seen immediately the release-box was opened. It seems possible that a chick which has pecked six times in rapid succession at food on a tray will 'associate' the sight of the tray with food more strongly than will a chick which has only pecked once, and that this kind of association may operate even when the tray is seen from a totally different aspect (*e.g.* from the release-box). On the other hand, if there is any influence of the amount of the reward on maze learning, then this influence must be due to quite another kind of 'association'—namely, associations between objects which are seen (or actions which are performed) *before* the food is reached and food.

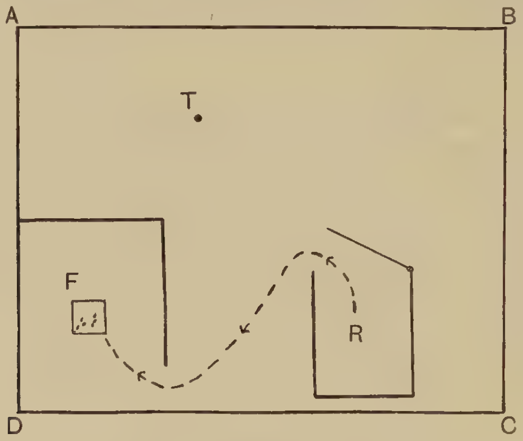


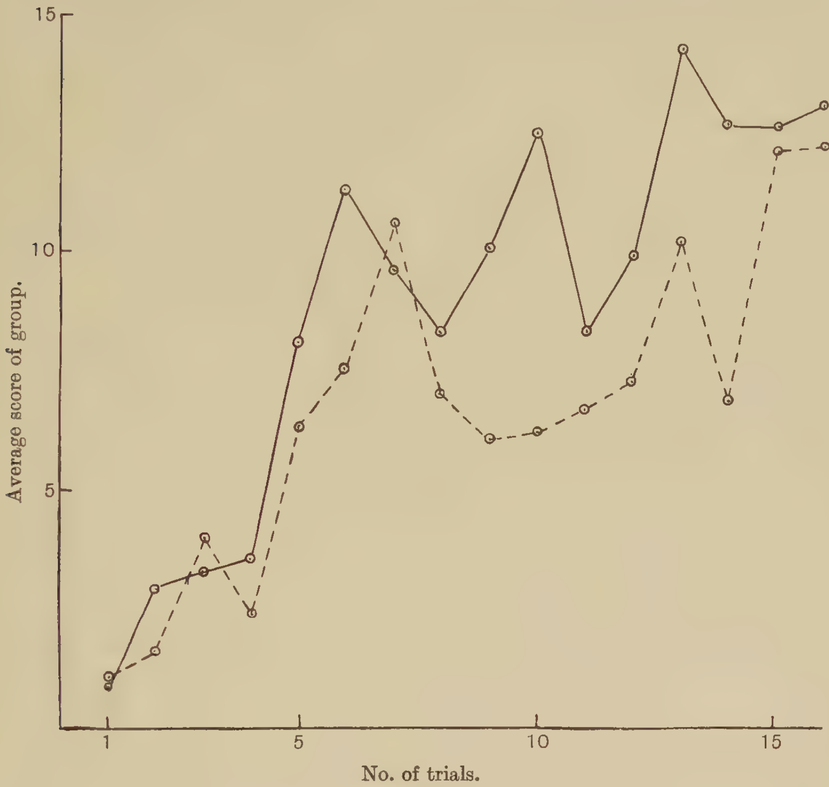
Fig. 1.

An experiment was therefore carried out with a very simple form of maze. A plan of the apparatus is shown in Fig. 1, the enclosure *ABCD* measuring about 5 ft. × 4 ft. *R* is the release-box, *F* is the food-tray, and *T* is the position of two tethered chicks. There was no preliminary training.

Two groups, each of ten chicks, were employed. The chicks of Group 1 received one grain of rice, and those of Group 6, six grains. The experiments began on the twenty-seventh day after hatching and continued until the thirty-second day. Care was taken to ensure that the chicks of the two groups had, as nearly as possible, equal appetites for rice at each test.

The learning curves are shown in Curves 2 A, scores being reckoned as before. The difference between the total scores of Group 6 and Group 1 is about 29 per cent.

The experiment was repeated (with certain minor modifications which need not be described) with another twenty chicks. The results are given in Curves 2 B. The difference between the total scores is in this case about 23 per cent.

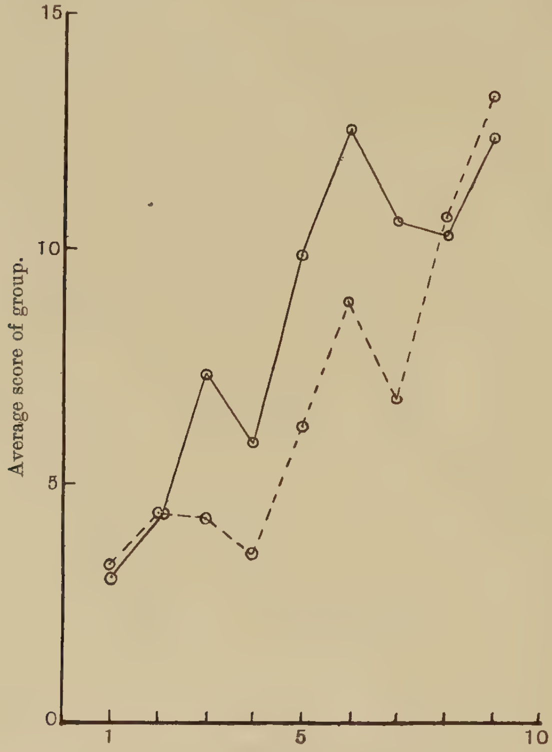


Curves 2 A.
——— Group 6. - - - - Group 1.

All the chicks used in these experiments retained their appetite for rice, and none was disqualified. The chicks receiving a reward of six grains were not noticed, on the whole, to be more excited during the experiments than those receiving only one grain. But there were wide individual differences, and judgment on this question could not be made with any certainty.

4. Exp. 3: Learning with no reward.

In this experiment the apparatus used was that of Exp. 1, but a flat piece of wood with a few grains of rice on it was substituted for the food-tray, and a sheet of plate-glass was placed across the passage a few inches from this, so that the chicks could see but could not reach the food. The chicks were experimented on in pairs, and tethered chicks were therefore not required.



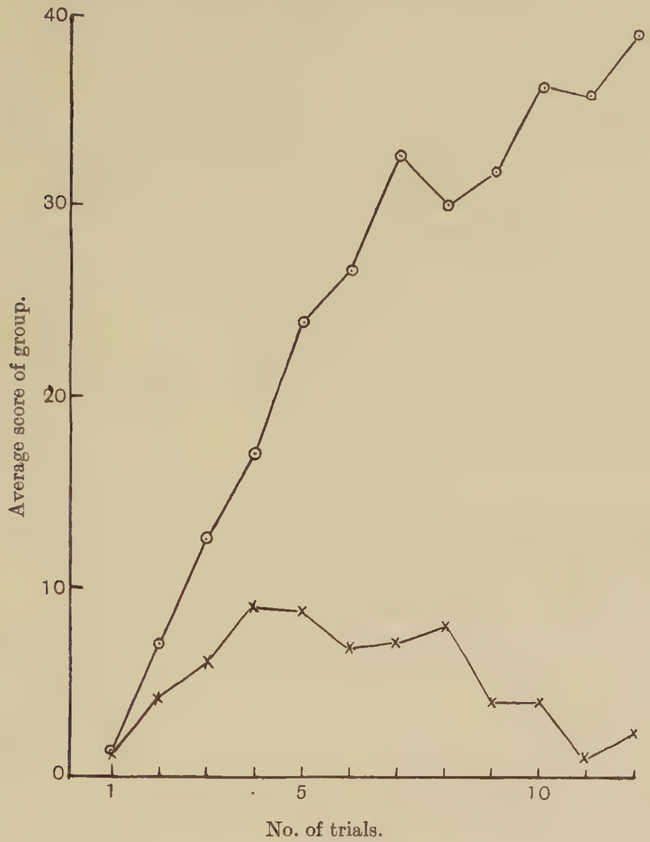
No. of trials.

Curves 2 B.

——— Group 6. – – – – Group 1.

Learning curves were obtained in the usual way, the scores being based on the time elapsing between the opening of the release-box and the instant when the slower of the two chicks reached a point a few inches from the glass. Each pair of chicks was given twelve trials in rapid succession. In a control experiment with other pairs of chicks the glass was removed (so that the chicks could reach the food), but the conditions were otherwise identical. The experiments were performed on the twenty-fifth day after hatching.

The results are shown in Curves 3. Curve A shows the average scores of five pairs of chicks receiving no reward, and Curve B shows the average scores of five pairs of chicks allowed to reach the food.



Curves 3.

× Curve A (No Reward). ⊙ Curve B (Reward).

It will be seen that the chicks receiving no reward improved for several trials and then grew worse. This result might be expected on grounds of common sense, but it is difficult to explain on some of the current theories of learning. The initial rise of the curves shows that the occurrence of the 'consummatory response' is not essential to the early stages of learning. The subsequent fall suggests a process analogous to

Pavlov's 'extinction' of conditioned reflexes (6), and it may be taken as further evidence of the inadequacy of mere repetition in the formation of habits.

5. CONCLUSIONS.

(1) The results of Exps. 1 and 2 support the view that, under the conditions of these experiments, the amount of the reward has some influence on the rate of learning. In Exp. 2, where the conditions were those of simple maze learning, it was found that a sixfold increase in the amount of the reward produced an increase of about 25 per cent. in the rate of learning. But it must be admitted that the individual differences between the chicks were so great that the experiment (although forty chicks took part in it) was not sufficiently accurate to establish this result with certainty.

(2) Exp. 4 shows that, under certain conditions, a transient kind of learning occurs when no reward is given.

I wish to express my thanks to Professor A. P. Chattock and Mr M. H. Carré for suggestions and help.

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(*Manuscript received* 1 *December* 1928.)

THE QUANTITATIVE ESTIMATION OF THE SENSATION OF COLOUR

By R. S. MAXWELL, M.A., B.Sc.

1. *Introduction* (pp. 181–182).
2. *Preparations and experimental work* (pp. 182–184).
    - (*a*) *Preparation of the discs* (p. 182).
    - (*b*) *The observers* (p. 183).
    - (*c*) *Conduct of the experiments* (p. 183).
    - (*d*) *Conversion to numbers* (p. 184).
3. *Analysis of results* (pp. 184–187).
    - (*a*) *Relationship between percentage angle of red and mental estimate of pink* (p. 184).
    - (*b*) *Miscellaneous discs of* 80 *per cent. colour* (p. 187).
4. *Correlation* (p. 188).
5. *Photometer measurements* (pp. 188–189).
6. *Acknowledgments* (p. 189).
7. *Summary* (p. 189).

1. INTRODUCTION.

L. F. RICHARDSON[1], in his paper on "Quantitative Mental Estimates of Light and Colour," describes some experiments in which he obtained the opinions of a large number of persons as to the degree of redness in a certain pink. In the analysis of the results, the subjects were classified in different groups, *e.g.* Fellows of the Royal Society, students working for the Intermediate B.Sc., etc. It was found that for groups containing eight or more members, the group-mean was approximately constant for the same sex. Therefore it appeared that in any subsequent research it would be sufficient to work with one group provided that it was not too small.

Richardson stated concerning his experiments: "the pink, made by mixing $\frac{2}{3}$ of scarlet with $\frac{1}{3}$ of white on a colour wheel, was judged mentally to be at the following positions on a scale in which 0 is white, 100 is scarlet. Mean estimate by men 43, by women 50. There is thus a remarkable disagreement between angles and mental estimates."

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[1] This *Journal*, 1929, xx (1), 27–37.

This present research was undertaken in order to investigate further the relationship between the angular percentage of red in the colour wheel and the mental estimate of the position of the pink on the scale of redness.

2. PREPARATIONS AND EXPERIMENTAL WORK.

(*a*) *Preparation of the discs.*

A series of colour wheels were prepared by mounting red blotting paper (Ford's blotting—428 mill—red) on some pure white "mounting board," with Kodak photographic mounting paste. The blotting paper was cut into the shapes shown in Fig. 1. The diameter of the white card was 30 cm., that of the larger red circle was 22·5 cm., and that of the smaller red circle was 15 cm. Table I shows the angle and the percentage angle of red in the middle zone of the disc.

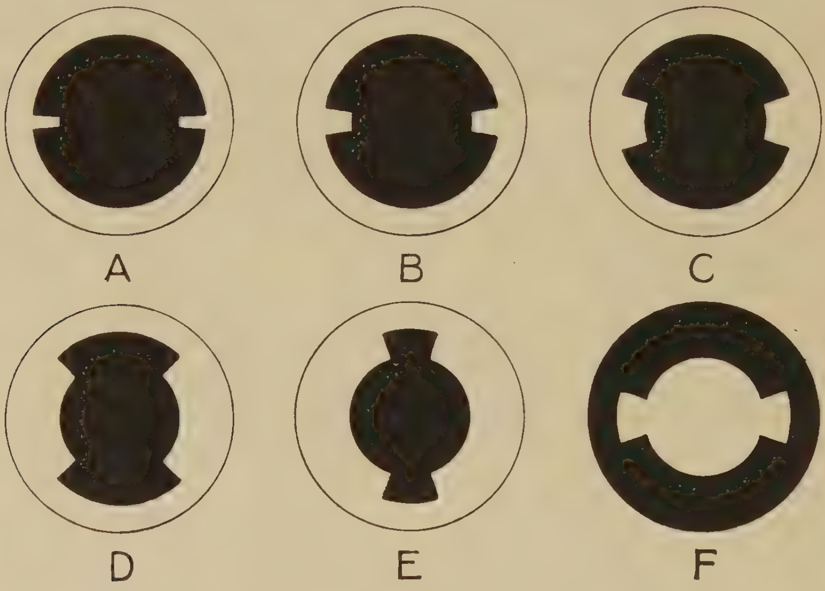

Fig. 1.

Disc H is just like Disc C except that the centre is Blue instead of Red.

The pink coloration of the middle zone is produced when the disc is mounted on an electric motor, and rotated at a sufficiently high speed to obviate any flicker due to the rapidly alternating patches of red and white.

Table I.

Identifying mark of disc	*A*	*B*	*C*	*D*	*E*	*F*	*H*
Angle of red on the disc in degrees. Remainder of 360° is white	342	324	288	180	72	288	288
Percentage angle of red in disc	95	90	80	50	20	80	80

(*b*) *The observers.*

The group of observers who made the estimates of the pink of the discs were boys from the upper forms of Manor House School, Clapham Common, S.W. 4. The ages of the boys ranged from 14 to 18 years, the average age being 15·8 years.

No relationship between age and ability to make reasonable estimations could be observed, the younger boys being quite as able to grasp what was wanted of them, and to do it, as those who were older.

The experiments were conducted on four separate days, and it was not possible for all the boys to be present each time. Thirty-five boys in all made estimations, but the number present on any one day was thirty.

(*c*) *Conduct of the experiments.*

The boys were seated in a class-room which was well lighted with four windows. All the days of the experiments were bright, but no direct sunlight came into the room. The motor on which the discs were rotated stood on a desk facing the boys and it could be seen comfortably by all. The class were only allowed to see the discs while they were in rotation, and when it was necessary to change a disc a cloth was held up which completely screened the disc from the boys.

The distance of the boys from the disc varied between 1 and 7 metres, but on analysis of the results no connection between distance and estimation of pink could be observed.

The method adopted for recording the observers' estimates was the same as that used in Richardson's experiments with the exception that there were no written instructions. Each boy was given a slip of paper on which was drawn a line 10 cm. in length with 'RED' written at one end and 'WHITE' at the other. These slips were reproduced by means of a hectograph. Brief oral instructions were given, care being taken to emphasize that the individual opinions of the boys were required, that each opinion was as good as any other, and that there was no 'right answer.' The boys were told to make a mark on the line, where they felt that the pink came on the scale of redness between the white and the red. No one complained that the task was impossible. The slips were collected after each disc had been shown, so that the boys could obtain no information from their own previous estimates, except by memory.

The actual experiments took place on four different days, and on each day five discs were shown to the class. Since the main object of the research was to find the relationship between the angular percentage of red and the mental estimate of the shade of pink produced, the usual reversals were made to eliminate fatigue and practice. The series of discs *A–E* was shown on three occasions at intervals of four days; the details may be seen by reference to Table II. On the other day observations were made on miscellaneous discs having a central zone of 80 per cent. colour. Further particulars will be found in Table III.

The results of all these experiments are discussed in the next section.

(*d*) *Conversion to numbers.*

The slips were measured by the author and the measurements converted into numbers. Following Richardson, the 'estimate' of the pink is defined to be "the percentage of the length of the line from the end marked white up to the mark." The scale is:

White = 0 Red = 100

and since the total line is 10 cm. in length, it was only necessary to measure the portion from the end marked WHITE to the mark in millimetres. "Throughout the arithmetic, numbers are rounded off to the nearest unit percentage, or if exactly midway, then to the neighbouring integer which is even."

3. ANALYSIS OF RESULTS.

(*a*) *Relationship between percentage angle of red and mental estimate of pink.*

The results of the experiments made on the first, third and fourth days are summarized in Table II.

The following points emerge from a consideration of that table:

(1) The distribution of estimates is fairly regular. In no case does the median differ from the mean value by more than two units.

(2) The standard deviations from the mean value are less for the extreme shades, *i.e.* for the very pale pink and the very deep pink, than for the intermediate shades. This fact was very noticeable when measuring the slips. Most of the marks made by the boys showed very good agreement among themselves for these extreme shades, but there were many rather wild guesses made when a disc such as *C* was shown.

(3) There is a definite dependence upon the order in which the discs are shown.

A possible explanation is as follows: On the first and fourth days disc *A* was shown first, and this was a very deep pink. The feeling was to make a mark at a certain distance from the *red* end of the line. On the third day, disc *E*—a very pale pink—was shown first. In this case, the mark was made at a certain distance from the *white* end.

Table II.

Number of boys who made estimates each day = 30.

Date	Sequence in which discs were shown to class	Estimates	Identifying mark of disc				
			A	*B*	*C*	*D*	*E*
			Percentage angle of red in disc				
			95	90	80	50	20
1928 13 July, 10 a.m.		Greatest	94	83	67	40	14
		Median	86	66	50	23	7
		Mean	**86**	**66**	**48**	**24**	**8**
	→	Least	72	44	12	8	1
		Standard deviation of individual from mean of class	6	9	12	8	4
1928 17 July, 10 a.m.		Greatest	99	96	84	48	41
		Median	94	82	67	30	10
		Mean	**94**	**83**	**67**	**32**	**11**
	←	Least	86	65	52	18	3
		Standard deviation of individual from mean of class	3	8	10	9	8
1928 21 July, 10 a.m.		Greatest	98	91	74	44	24
		Median	90	73	50	20	6
		Mean	**89**	**72**	**48**	**21**	**8**
	→	Least	75	51	31	9	2
		Standard deviation of individual from mean of class	6	10	10	8	5
General Mean	$\frac{1}{2}[\frac{1}{2}(\rightarrow + \rightarrow) + \leftarrow]$		**91**	**76**	**58**	**27**	**10**

The general tendency seemed to be to mark off rather a large difference for the first one and then to 'push on' further for each succeeding disc.

In any case the values obtained when proceeding from pale pink to deep pink were consistently higher than those obtained when the discs were shown in the other sequence.

(4) The results obtained when the first series of experiments was repeated again on the fourth day, show good agreement between the two sets of observations. It thus appears that the effect of practice in making these mental estimates is not so pronounced as the effect of sequence.

(5) For all shades of pink the mental estimate is considerably lower than percentage of red actually contained in the red and white of the colour wheel.

A given amount of white thus appears to have more influence in governing the shade of the resulting pink than does the same amount of red. This result is in agreement with the photometric measurements described in section 5.

The graphical relationship between the percentage angle of red and the mental estimate is shown in Fig. 2.

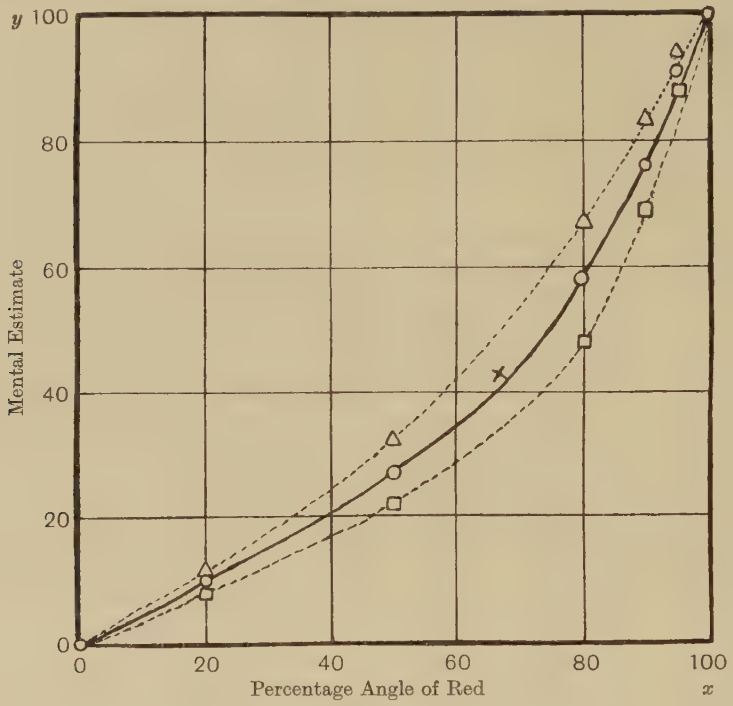

Fig. 2.

□ Mean of values obtained on July 13 and 21.
△ Values obtained on July 17.
○ General Mean values.
× Richardson's value for men.

It has been found that if

$$\text{Percentage angle of red} = x,$$

and

$$\text{Mental estimate} = y,$$

then the mean values of the experimental points lie on the hyperbola

$$(x - 156)\,(y + 56) = -\,8736.$$

It will be noticed that the mean value obtained by Richardson for men using a slightly different colour red paper also lies very close to this mean curve.

(*b*) *Miscellaneous discs of* 80 *per cent. colour.*

The experiments on the second day were made in order to test the effect of (α) an interchange in the positions of the red and the white in disc C, and (β) the use of a different colour—in this case blue.

Table III shows the tabulated results of these experiments.

It will be noticed that:

(1) The standard deviations for all the experiments made on this day were fairly large, many erratic estimates being made.

(2) The two estimates for disc F, namely 45 and 53 (mean 49) are lower than those for disc C namely 55 and 51 (mean 53).

Probably in order to obtain a true mean value for the estimates, a second set of discs ought to be prepared of the type of F, and two further sets of readings taken, one for each sequence of discs.

(3) The estimate for the blue disc H (60) is higher than any other estimate made on this day, but it lies between the values for the red disc C found on the third day (67) and the first and fourth days (48).

Table III.

Number of boys who made estimates = 30.

Date	Sequence in which discs were shown to class	Estimates	Identifying mark of disc				
			F	C	H	C	F
			Description of disc				
			Red rim white centre	White rim red centre	White rim blue centre	White rim red centre	Red rim white centre
1928		Greatest	81	80	78	83	60
14 July,		Median	52	52	58	51	44
9 a.m.		**Mean**	**53**	**55**	**60**	**51**	**45**
	→	Least	30	33	38	32	23
		Standard deviation of individual from mean	10	11	10	11	9

In order to study the effect of colour in any detail, a large number of further experiments would have to be made. Probably the most fundamental results would be obtained on working with black and white, and various colours could then be tried afterwards. The reason why the present writer used red was because Richardson had done so, and it was deemed desirable to extend his experiments. The reason that Richardson chose red was to provide an answer to the statement of William James[1]. "To introspection, our feeling of pink is surely not a portion of our feeling of scarlet."

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[1] W. James, *Principles of Psychology*, 1901, I, 546.

4. Correlation.

It seemed to be of interest to obtain some information concerning the persistence of individual differences, *i.e.* if a boy made an estimate much higher than the mean value for one disc, whether or not he did so for all the others. It would have taken too long to work out all the possible combinations of correlation coefficients for the various discs, but Table IV gives some sample coefficients calculated by the product moment method from the first set of observations made 13 July.

Table IV also shows the correlation coefficients obtained from the experiments of the second day. They are much lower than those found in the sequence. This may be attributed to the fact that all the discs were of 80 per cent. colour and the standard deviations were high.

It was not convenient to make a correlation between estimates of the same disc made on different days, because the boys present varied slightly from day to day.

Table IV.

Date	Discs	*A* and *B*	*B* and *C*	*C* and *D*	*D* and *E*
1928	Correlation coefficient	$0{\cdot}3_2$	$0{\cdot}6_0$	$0{\cdot}7_5$	$0{\cdot}7_1$
13 July	P.E. of correlation	$0{\cdot}1_1$	$0{\cdot}0_8$	$0{\cdot}0_5$	$0{\cdot}0_6$
	Discs	*F* and *C*	*F* and *F*	*C* and *C*	*H* and *C*
1928	Correlation coefficient	$0{\cdot}1_4$*	$0{\cdot}3_5$	$0{\cdot}1_6$*	$0{\cdot}2_4$
14 July		$0{\cdot}4_7$	—	—	$0{\cdot}3_1$
	P.E. of correlation	$0{\cdot}1_2$	$0{\cdot}1_1$	$0{\cdot}1_2$	$0{\cdot}1_2$
		$0{\cdot}1_0$	—	—	$0{\cdot}1_1$

* These two correlations are both exceptionally low. This is due to a very erratic estimate by one boy of the disc *C*. His two estimates of this disc in this series of observations were 75 and 32! If his values are omitted, the two correlation coefficients both become $0{\cdot}2_9$.

5. Photometer measurements.

It was considered desirable to make measurements on the relative reflectivities of the white card and the red blotting paper.

Since it is impossible to make such a comparison in white light, red filters were employed. The wave-length of the red light used was about 6500 Ångström units.

Two sets of experiments were made. For the first series of observations a Richardson aeroplane photometer[1] was adapted.

In the second set of experiments, which were conducted in a darkened room, the distances of the two reflecting surfaces from a lamp were adjusted until the two surfaces reflected equal amounts of light to the observer. The ratio of the reflectivities was then equal to the ratio of the squares of the distances of the surfaces from the lamp.

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[1] Union Géodésique et Géophysique Internationale, Section de Météralogie "Report on photometers for a survey of the reflectivity of the earth's surface," 1928, Camb. Univ. Press.

Let $$R = \frac{\text{reflectivity of white card}}{\text{reflectivity of red blotting paper}}.$$

Then by photometer measurements $R = 1{\cdot}5_6$,
and by distance measurements $R = 1{\cdot}5_8$.

Thus the white card reflects $1{\cdot}5_7$ times as much red light as does the red blotting paper.

6. ACKNOWLEDGMENTS.

The author's thanks are due to all the boys who made the observations; to the Head Master of Manor House School, Mr S. Maxwell, M.A., LL.B., for permission to carry out the experiments in the school; and to the Science Master, Mr T. R. Allitt, B.Sc., both for assistance during the conduct of the experiments in the class-room, and for help in some of the photometric measurements. Also in a special degree the author wishes to express his gratitude to Dr L. F. Richardson, F.R.S., for much helpful advice and criticism throughout the research.

7. SUMMARY.

Quantitative mental estimates of the amount of red present in the pink of certain colour wheels were made by thirty-five boys of average age 15·8 years. The different pinks were produced by varying the percentage of red and white on the colour wheels. If x = percentage amount of red in colour wheel and y = mental estimate of pink, it has been found that the functional relationship between x and y is represented by the hyperbola

$$(x - 156)(y + 56) = -8376.$$

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BOUNDARY CONDITIONS FOR CORRELATION COEFFICIENTS

BY P. J. DANIELL.

IN a recent paper[1] Dr J. Ridley Thompson discusses the boundary conditions which must be satisfied (in the case of 3 and also of 4 variables) if certain measures can be regarded as combinations of specific factors together with 'first order group factors'; that is to say, factors each common to 2 variables only. The present paper gives a simpler method of attack which permits an immediate extension to n variables, and to the case of factors each common to p of the n variables. The results in the case of 3 and of 4 variables are the same as Thompson's results.

We also obtain a rough rule that for n measures with factors each common to p of them the *average coefficient of correlation must not exceed* $(p-1)/(n-1)$. Thus if there are 9 variables and their average coefficient of correlation is 0·6 there will be required factors common to at least 6 of them.

Three variables.

Let q_1, q_2, q_3 be the given measures, x_{12} a factor common to q_1, q_2, x_{13} common to q_1, q_3 and x_{23} common to q_2, q_3. In addition y_1, y_2, y_3 are specific factors. All of these variables are taken of unit standard deviation. Then

$$\left.\begin{aligned} q_1 &= a_{12}x_{12} + a_{13}x_{13} + b_1y_1 \\ q_2 &= a_{21}x_{12} + a_{23}x_{23} + b_2y_2 \\ q_3 &= a_{31}x_{13} + a_{32}x_{23} + b_3y_3 \end{aligned}\right\} \qquad \ldots\ldots(1),$$

in which a_{12}, a_{21}, for example, are not to be confused. It can be shown that these are equivalent to those [p. 80 (5)] of Thompson. Since q_1, q_2, q_3 are of unit deviation,

$$\left.\begin{aligned} a_{12}^2 + a_{13}^2 &= 1 - b_1^2 \\ a_{21}^2 + a_{23}^2 &= 1 - b_2^2 \\ a_{31}^2 + a_{32}^2 &= 1 - b_3^2 \end{aligned}\right\} \qquad \ldots\ldots(2),$$

and the correlation coefficients are given by

$$a_{12}a_{21} = r_{12}, \quad a_{13}a_{31} = r_{13}, \quad a_{23}a_{32} = r_{23} \qquad \ldots\ldots(3).$$

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[1] J. R. Thompson, this *Journal*, 1928, XIX, 87, also *ibid*, 1929, XIX, 239.

Our problem is to determine the inequalities which must be satisfied by r_{12}, r_{13}, r_{23} if these 6 equations are to be soluble for the 9 unknowns $a_{12}, a_{21}, \ldots, b_1, b_2, b_3$.

Let t_1, t_2, t_3 be unspecified variables. The quadratic form

$$t_1^2 + t_2^2 + t_3^2 - 2r_{12}t_1t_2 - 2r_{13}t_1t_3 - 2r_{23}t_2t_3 \qquad \ldots\ldots(4)$$

$$= (a_{12}^2 + a_{13}^2 + b_1^2)\, t_1^2 + \ldots + \ldots - 2a_{12}a_{21}t_1t_2 - \ldots - \ldots$$

$$= b_1^2t_1^2 + b_2^2t_2^2 + b_3^2t_3^2 + (a_{12}t_1 - a_{21}t_2)^2 + (a_{13}t_1 - a_{31}t_3)^2 + (a_{23}t_2 - a_{32}t_3)^2,$$

can never be negative for any real values of t_1, t_2, t_3. Hence, by a well-known property of quadratic forms the determinant

$$\begin{vmatrix} 1 & -r_{12} & -r_{13} \\ -r_{12} & 1 & -r_{23} \\ -r_{13} & -r_{23} & 1 \end{vmatrix} \geqslant 0 \qquad \ldots\ldots(5),$$

that is to say,

$$1 - r_{12}^2 - r_{13}^2 - r_{23}^2 - 2r_{12}r_{13}r_{23} \geqslant 0 \qquad \ldots\ldots(6).$$

This is Thompson's boundary condition for three variables. Furthermore, the equality sign can only arise when it is possible to choose t_1, t_2, t_3 (not all zero) so as to make the quadratic form take on the value 0. This happens only when

$$b_1 = b_2 = b_3 = 0, \quad a_{12}t_1 = a_{21}t_2, \text{ etc.}$$

Multiplying the three last equations together we see that

$$a_{12}a_{23}a_{31} = a_{21}a_{32}a_{13} \qquad \ldots\ldots(7).$$

This is Thompson's equation (10), $ABC = 1$.

It remains to prove the converse that if the boundary condition (6) is satisfied real solutions of equations (2) (3) can be found.

Let t_1, t_2, t_3 be quantities as yet undetermined and choose

$$a_{12} = \sqrt{(r_{12}t_2/t_1)}, \quad a_{21} = \sqrt{(r_{12}t_1/t_2)}, \text{ etc.}$$

Then $a_{12}a_{21} = r_{12}$, etc., and equations (3) are automatically satisfied. Equations (2) now become

$$r_{12}t_2/t_1 + r_{13}t_3/t_1 = 1 - b_1^2, \text{ etc.}$$

These again can be written

$$(1 - b_1^2)\, t_1 - r_{12}t_2 - r_{13}t_3 = 0, \text{ etc.} \qquad \ldots\ldots(8).$$

These three homogeneous equations in t_1, t_2, t_3 can certainly be satisfied (with not all t's equal to 0) if the determinant of the equations is zero, that is, if

$$\begin{vmatrix} 1 - b_1^2 & -r_{12} & -r_{13} \\ -r_{12} & 1 - b_2^2 & -r_{23} \\ -r_{13} & -r_{23} & 1 \end{vmatrix} = 0 \qquad \ldots\ldots(9).$$

This again will be the case if the quadratic form

$$t_1^2 + t_2^2 + t_3^2 - 2r_{12}t_1t_2 - 2r_{13}t_1t_3 - 2r_{23}t_2t_3 - b_1^2t_1^2 - b_2^2t_2^2 - b_3^2t_3^2$$

is never negative but is 0 for a certain choice of the ratios of t_1, t_2, t_3. For this purpose it is sufficient to choose

$$b_1^2 = b_2^2 = b_3^2 = b^2,$$

where b^2 is the minimum value of form (4) subject to the condition

$$t_1^2 + t_2^2 + t_3^2 = 1.$$

Hence, if form (4) is never negative, the problem of finding factors of the kind required is soluble. Sufficient conditions are that

$$1, \quad \begin{vmatrix} 1 & -r_{12} \\ -r_{12} & 1 \end{vmatrix} \quad \text{and} \quad \begin{vmatrix} 1 & -r_{12} & -r_{13} \\ -r_{12} & 1 & -r_{23} \\ -r_{13} & -r_{23} & 1 \end{vmatrix}$$

should all be non-negative. Of these the first two conditions are *always* satisfied so that the third condition is sufficient by itself.

n variables.

This repeats the case of 3 variables and with an obvious notation

$$q_i = \Sigma_j a_{ij}x_{ij} + b_iy_i \qquad \text{......(10).}$$

It follows that

$$\Sigma_j a_{ij}^2 = 1 - b_i^2 \qquad \text{......(11),}$$

$$a_{ij}a_{ji} = r_{ij} \qquad \text{......(12).}$$

The quadratic form

$$\Sigma_i t_i^2 - \Sigma_{i,\,j} r_{ij}t_it_j \qquad \text{......(13)}$$

$$= \Sigma_i b_i^2t_i^2 + \tfrac{1}{2}\Sigma_{i,\,j}(a_{ij}t_i - a_{ji}t_j)^2 \qquad \text{......(14)}$$

is non-negative for all t_i. [Note that in the summation over all pairs i, j a term like $r_{12}t_1t_2 = r_{21}t_2t_1$ will appear twice.] Then we must have

$$\begin{vmatrix} 1 & -r_{12} & -r_{13} & -r_{14} & \cdots \\ -r_{12} & 1 & -r_{23} & -r_{24} & \cdots \\ \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & 1 \end{vmatrix} \geqslant 0 \qquad \text{......(15).}$$

When we consider the converse, as in the case of 3 variables, it is found that the necessary and sufficient conditions for solubility in terms of first order group factors is that all the determinants,

$$\begin{vmatrix} 1 & -r_{12} & -r_{13} \\ -r_{12} & 1 & -r_{23} \\ -r_{13} & -r_{23} & 1 \end{vmatrix}, \quad \begin{vmatrix} 1 & -r_{12} & -r_{13} & -r_{14} \\ -r_{12} & 1 & -r_{23} & -r_{24} \\ -r_{13} & -r_{23} & 1 & -r_{34} \\ -r_{14} & -r_{24} & -r_{34} & 1 \end{vmatrix}, \quad \ldots.$$

up to the nth order should be non-negative.

The 4th order determinant yields Thompson's condition for 4 variables but in addition 3 variables must also satisfy the 3-variable condition.

n variables, factors common to p at a time.

The n variables are $q_1, q_2, \ldots q_n$ and the specific factors $y_1, y_2, \ldots y_n$ as before. The group factors are called $x_{ijkl\ldots}$. Thus

$$q_i = \frac{1}{(p-1)!} \Sigma_{jkl\ldots} a_{ijkl\ldots} x_{ijkl\ldots} + b_i y_j \qquad \ldots\ldots(16)$$

and there are $p-1$ variable suffixes $jkl\ldots$ which can be interchanged in $(p-1)!$ ways. Then

$$\frac{1}{(p-1)!} \Sigma_{jkl\ldots} a^2{}_{ijkl\ldots} = 1 - b_i^2 \qquad \ldots\ldots(17),$$

$$\frac{1}{(p-2)!} \Sigma_{kl\ldots} a_{ijkl\ldots} a_{jikl\ldots} = r_{ij} \qquad \ldots\ldots(18),$$

$$\frac{1}{2}\frac{1}{(p-2)!} \Sigma_{ijkl\ldots} (a_{ijkl\ldots} t_i - a_{jikl\ldots} t_j)^2$$
$$= - \Sigma_{ij} r_{ij} t_i t_j + \frac{1}{(p-2)!} \Sigma_{ijkl\ldots} a^2{}_{ijkl\ldots} t_i^2.$$
$$= - \Sigma_{ij} r_{ij} t_i t_j + (p-1)(\Sigma t_i^2 - \Sigma b_i^2 t_i^2).$$

Therefore in this case the quadratic form to be considered is

$$(p-1)\Sigma_i t_i^2 - \Sigma_{ij} r_{ij} t_i t_j \qquad \ldots\ldots(19),$$

and the required condition[1] is that

$$\begin{vmatrix} (p-1) & -r_{12} & -r_{13} & -r_{14} & \cdots \\ -r_{12} & (p-1) & -r_{23} & -r_{24} & \cdots \\ \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & (p-1) \end{vmatrix} \geqslant 0 \ \ldots\ldots(20),$$

and that the corresponding determinants formed with the first 3, the first 4, the first 5, etc. variables shall all be non-negative.

Approximations.

These formulae are somewhat unwieldy when n is at all large. Moreover, the boundaries will be made blunter by the errors of sampling. Hence an approximation may be of considerable value. In such an approximation it is clear from the conditions that unless p is nearly equal to n the correlations must on the whole be small. For rough purposes we proceed as follows:

Let s be the average of the r's and let

$$r_{ij} = s(1 + \epsilon_{ij}).$$

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[1] Another form of this determinant is stated in J. R. Thompson, *Proc. Roy. Soc. Edinburgh*, 1928, XLIX, 66.

We shall suppose that s and ϵ are so small that terms of the 5th order in their products are negligible. Then in terms of the 4th order in r we can replace r_{ij} by s, and even in terms of the 3rd order in r we can also do this because of the symmetry of the expressions and because the average of ϵ is zero.

If we now denote the standard deviation of ϵ by δ the determinant in (20) can be expanded in the form

$$(p-1)^n - {}_nC_2\,(p-1)^{n-2}s^2\,(1+\delta^2) - 2\,{}_nC_3\,(p-1)^{n-3}s^3 \\ + 3\,{}_nC_4\,(p-1)^{n-4}s^4 - 6\,{}_nC_4\,(p-1)^{n-4}s^4 \geqslant 0.$$

Let $$\frac{n-1}{p-1}\,s = f,$$

then

$$\frac{n}{n-1}\,\tfrac{1}{2}f^2\,(1+\delta^2) + \frac{n\,(n-2)}{(n-1)^2}\,\tfrac{1}{3}f^3 + \frac{n\,(n-2)\,(n-3)}{(n-1)^3}\,\tfrac{1}{8}f^4 \leqslant 1.$$

When n is large this becomes

$$\tfrac{1}{2}f^2 + \tfrac{1}{3}f^3 + \tfrac{1}{8}f^2\,(f^2 + 4\delta^2) \leqslant 1.$$

If $\delta = 0$, f must be less than 1·05, while if $f = 1$, δ^2 must not exceed $\frac{1}{12}$. Hence it can be taken as a rough rule that $f < 1$ or that

$$\text{average } r < (p-1)/(n-1).$$

In addition to this condition, a similar condition—with the same p—must be satisfied for any number, m, of the variables chosen from the n variables.

As a test of this rough rule we can consider the extreme case of 3 variables as exactly as possible. In this case let us write

$$r_{12} = x, \quad r_{13} = y, \quad r_{23} = z.$$

On the boundary

$$x^2 + y^2 + z^2 + 2xyz = 1,$$

and we wish to find the maximum value of $x + y + z$ when this condition is satisfied. On working it out it is seen that this maximum is obtained when

$$x = y = z = \tfrac{1}{2},$$

and then $$\text{average } r = \tfrac{1}{2} = (2-1)/(3-1).$$

Hence our rough formula is exactly satisfied in the case of 3 variables. It is not, however, an exact *sufficient* condition for the analysis into first order group factors to be possible.

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PUBLICATIONS RECENTLY RECEIVED

Industrial Psychology. Edited by CHARLES S. MYERS. London: Thornton Butterworth (Home University Library). 1929. Pp. 248. 2*s.* 6*d.* net.

For anybody who wants to get a general idea of what industrial psychology is accomplishing in this country this is certainly about the best book available. The introduction, written by Dr Myers, sets forth clearly the purposes, problems and the general basis of industrial psychology. The essays that follow, with the exception of the one by Dr Drever on "The Human Factor in Industrial Relations," are all contributed by investigators of the National Institute of Industrial Psychology. They are uniformly clear, interesting, and represent a great amount of first-hand work. The subjects dealt with are: Work and Environment, by A. M. Hudson Davies; Work and Rest, by Rex Knight; Ease and Speed of Work, by G. H. Miles and A. B. B. Eyre; Unproductive Working Time, by A. Angles; Industrial Accidents, by A. Stephenson; The Measurement of Intelligence and Aptitudes, by F. M. Earle; Choosing a Career, by A. Macrae; Square Pegs and Square Holes, by Winifred Spielman; Industrial Psychology and Welfare Work, by Sheila Bevington; The Economic Aspects of Industrial Psychology, by F. W. Lawe; Industrial Psychology and Agriculture, by W. R. Dunlop.

The Opium Problem. By C. E. TERRY and MILDRED PERRENS. New York. 1928. Pp. xvi + 1042.

This important book is published by the American Committee on Drug Addictions in collaboration with the Bureau of Social Hygiene. Messrs Baillière, Tindall and Cox, of London, are acting as agents for the book in this country. No more comprehensive and authoritative study of the opium problem, in all of its many ramifications, has yet been published, or is likely to appear for a long time. The investigation is naturally mostly concerned with opium addiction in the United States of America, but psychologists, psychiatrists and social workers in every country will find here a mine of useful and significant information. First a survey is made as to the extent of opium taking in America and its increase from early days. Next comes a study of the history of opium from very early days to now. The third chapter deals with etiology, and this is followed by a discussion of the general character of the effects of opium and their relation to both physical and mental disease. A very thorough study of the pathology of opium results occupies two long chapters, the subject being dealt with under the head of Somatic and Psychic changes and the phases of tolerance, dependence and withdrawal being exhaustively considered. Symptomatology next occupies the attention, and this leads to a most interesting survey of the main Types of Users of opium. Various methods of treatment and their comparative values are described, and finally methods of control of the drug: international, national, state and municipal are presented. A very full bibliography and index complete the volume. The thoroughness of the survey should make this a standard book of reference.

Mysteries of the Soul. By RICHARD MULLER FREIENFELS, translated by BERNARD MIALL. London: George Allen and Unwin, Ltd. 1929. Pp. 348. 12*s.* 6*d.* net.

The translator has done his work extraordinarily well, and so far as its English style goes the book makes remarkably good and attractive reading. The actual material is throughout of philosophical and literary rather than of psychological interest. The topics dealt with are: The Infinity of the Soul, The Individual and his

Destiny, A Journey into the Past, The Dramaturgy of Life, The Americanization of the Soul, and The Religion of the Future. From a psychological point of view by far the most interesting of these chapters is the one in which the author tells part of the story of his childhood, though even here he cannot refrain from constant interpretation and comment in accord with the considered views of his later life, and the chapter in which he discusses the "americanization" of life. The latter chapter has many acute observations bearing upon some of the problems of social psychology. On the whole the book is one to be enjoyed rather than one to be studied.

The Science of Character. By LUDWIG KLAGES, translated by W. H. JOHNSON. London: George Allen and Unwin, Ltd. 1929. Pp. 308. 10*s.* 6*d.* net.

This is a work which has enjoyed considerable popularity in Germany, but it is far from easy to see why. No doubt everybody is now turning to a study of temperament and personality for the solution of his practical psychological problems. But the treatment of character in this book is long-winded, woolly and pretentious to a degree, and it is hard to see that the book has any claim to genuine originality of the more theoretical type. The translation is clear, but not particularly pleasant to read. However, the translator had undoubtedly a difficult task.

Colour and Colour Theories. By CHRISTINE LADD-FRANKLIN. London: Kegan Paul. 1929. Pp. xv + 284. 12*s.* 6*d.* net.

The Editor of the International Library of Psychology, Philosophy and Scientific Method has here collected together a very considerable number of Mrs Ladd-Franklin's articles and reviews of books. They are presented with a number of nicely produced colour diagrams most of which are as hypothetical as the theories which they illustrate. Readers may be glad to have these vigorous controversial papers all put together, though the repetition which the method involves is apt to get a bit wearisome.

The Growth of the Mind: an Introduction to Child Psychology. By K. KOFFKA. London: Kegan Paul. 1928. Pp. xix + 427. 15*s.* net.

It is a pleasure to have to record the appearance of a second edition of this stimulating and original book. A good many alterations have been made with a view to rendering the treatment at once more internally consistent and less controversial in tone. Moreover, more recent work of some of the Gestalt psychologists, especially the important researches of Lewin, receives consideration. The book as it stands is undoubtedly one of permanent value, not only for the educationist, but also for every serious student of psychology, and adds distinction to the well-known International Library of Psychology, of which it is one volume.

The Psychology of Philosophers. By ALEXANDER HERZBERG. London: Kegan Paul. 1929. Pp. viii + 228. 10*s.* 6*d.* net.

This volume of the International Library of Psychology discusses the psychological causes which predispose a man to a philosophic outlook. The treatment is in the main in general terms, though the study centres about the lives and work of thirty philosophers who are taken as characteristic. Various special topics are taken up in a series of short and lively chapters, and the author's main thesis is that the great thinkers are men with more than normally intense inhibitions. Many may think the book more than a little superficial, but few will find it uninteresting. The translator, E. B. F. Wareing, has done his work well.

Incentives to Study: A Survey of Student Opinion. By A. B. Crawford. Yale University Press (and Oxford University Press). 1929. Pp. xiii + 194. 23*s*. net.

This is a comprehensive account of the carefully collected opinions of a large number of students at Yale University concerning the motives and incentives that prompt them to carry through their academic work, and the College influences that are most important in shaping their careers. A good many of its findings have, no doubt, a mainly local significance, but the whole study is full of interest for all people who may be concerned with higher education. The book itself is produced with needless magnificence, and is, by consequence, unfortunately expensive.

The Development of the Human Eye. By Ida C. Mann. Cambridge University Press. 1928. Pp. x + 306. 36*s*. net.

There is no other book that covers the story of the development of the human eye with anything like the completeness that is achieved in this extremely valuable volume. As Sir John Parsons says in his introduction: "It has been possible to illustrate every stage of human ocular development by an actual specimen, independent of reference to other doubtfully related species. In this way certain debatable points, resting hitherto on comparative observations only, have been placed on a sounder foundation, and certain other processes for example, those involving growth rates and consequent alteration of position, have been studied for the first time on human material." The book is in fact a splendid record of patient original research, illustrated profusely, and is likely to remain for many years a standard work of reference. A very full and valuable bibliography is included.

The Cytoarchitectonics of the Human Cerebral Cortex. By C. von Economo, translated by S. Parker. Oxford University Press. 1929. Pp. xii + 186. 21*s*. net.

This is a clear and excellent translation of Professor von Economo's shorter and recent text-book. It will be of considerable value to anatomists, psychiatrists, some physiologists and here and there to a sufficiently trained psychologist. Those psychologists who are hardly able to follow the work in detail will be particularly interested in the final chapter with its important remarks on the mode of functioning of the cerebral cortex. "Just as the retina is sensitive to light in all its parts and yet needs its totality to produce an accurate picture, so is the intact condition of the whole cerebrum necessary to the execution of perfect conscious psychic functions." Yet in the brain there are centres much like that of the macula lutea in the eye, where certain functions come to a focus and are most potently exercised. There is thus a receptive focus in the posterior, an efferent focus in the anterior, portion of the cerebrum. The author's remarks about these and his speculations concerning their general development are of very great interest.

The Relation of the Skull and Brain to Crime. By W. Norwood East. Edinburgh: Oliver and Boyd. 1928. 6*d*. Pp. 28. Henderson Trust Lecture, No. VIII.

The general trend of this instructive lecture is historical. An account is given of the work of the phrenologists and of some alienists in the investigation of the skull and brain characteristics of criminals. Contemporary work in psychology, normal and abnormal, is also considered. No very definite conclusions emerge, but the lecturer is most sympathetic to the mode of approach due to Professor Cyril Burt.

Rhythm and Metre. By THOMAS TAIG. Cardiff: University of Wales Press Board. 1929. Pp. 140. 5*s*. net.

The subject of rhythm has, for obvious reasons, always held an attraction for the psychological experimenter. But most of his work has had little direct concern with the rhythms of word and sound as they appear in poetry, prose and music. The strong modern tendency to try to keep psychological investigation more closely in touch with real life must have led many students to consider a new experimental approach to the study of rhythm less dominated by the idea that all such investigation must attempt to deal only with the simplest and most perfectly controlled instances. For any such students, as well as for any reader whatever who is genuinely interested in rhythm in poetry, prose and music, this will prove an attractive book, raising far more problems than it even attempts to solve, but suggesting many interesting lines of study. The author, having settled his definitions, deals in two largely psychological chapters with Sound Rhythms and The Representation of Speech-Rhythms. The graphical method of representation illustrated in the second of these chapters, though it must be very difficult to write accurately, may appeal to many readers. Thereafter the author deals with: The Pattern of Verse and with The Spoken Line. He presents numerous and apt illustrations, and maintains with much force that the perception of rhythm is dependent upon specific pattern experiences the character of which calls for psychological rather than objective analysis. A final chapter on The History of Music and Verse tries to trace a continuous rhythm development in English metre, and to show how that development has been achieved. The book is a contribution of real value to the psychological study of rhythm.

Introduction to the Study and Interpretation of Drama. By J. W. KAISER. Amsterdam. 1929. Pp. 77.

"Drama," begins the author, "is a product of a psyche which unconsciously projects its arrangement towards a certain problem into an apparently multi-personal demonstration of life." This seems to mean that in dramatic composition the dramatist reveals much of his own character. From this point of view *Cyrano de Bergerac* and *The Merchant of Venice* are studied in considerable detail.

Modern Language Instruction in Canada. University of Toronto Press. 1928. Vol. I. Pp. xlviii + 547. Vol. II. Pp. 852.

These two large, comprehensive and excellently produced volumes are published by the American and Canadian Committees on Modern Languages, and claim the serious attention of all those who are interested in the teaching of the subjects dealt with. The first volume is chiefly occupied by an annotated bibliography of recent work on language teaching, together with a formulation of many problems still awaiting research. All the way through the emphasis is laid on a psychological method of approach. The second contains a history of modern language teaching in Canada, and a full account of the results of the application on a large scale of tests of language proficiency in Canada, England and America. Not only are the results themselves extremely interesting, but their main significance in regard to needs and methods of future work are set forth clearly and judiciously. The main part of Volume I is published separately under the title: *An Annotated Bibliography of Modern Language Methodology.*

Retention of History in the Sixth, Seventh and Eighth Grades with Special Reference to the Factors that Influence Retention. By SARAH JANE BASSETT. Johns Hopkins Studies in Education, No. 17. Baltimore: Johns Hopkins Press. 1928. Pp. viii + 110. $ 1.75.

This is a statistical and experimental study, together with a chapter on the history of work on retention and an annotated bibliography.

Phonophotography in folk music. American negro songs in new notation. By MILTON METFESSEL. The University of North Carolina Press. 1928. Pp. x + 181. $3.0.

The study of folk music by listening to phonographic records is manifestly unsatisfactory. The student tends to concentrate on those features with which his previous training has made him familiar; the unfamiliar tends to be neglected or misinterpreted owing to the elusiveness of the auditory perception. With the advent of phonophotography, by which means the impressions on the records are transformed into tracings on a photographic film, the study enters on a new phase. The idiosyncrasies of folk music, as well as those of the individual singers, are laid bare and can be studied at leisure. Differences of phrasing, of attack, of interpretation may be analysed and compared—a great advantage to those interested in the perceptual side of the psychology of music.

In this volume the methods of obtaining the photographs and of analysing the results are described, while the analysis of portions of many American negro songs are set out in a new "pattern notation," specially devised to bring out the various factors in the singing. Incidentally it is of interest to note that the finished film for a four minutes' record is estimated to cost $25.0, and its analyses an additional $75.0. The volume should be seen by all who are working in this branch of psychology or of aesthetics.

The Terror Dream. By G. H. GREEN, Ph.D., M.A., B.Sc. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1927. Pp. 126. 5*s*. net.

In this essay Dr Green considers the mechanism of the terror dream. According to his analysis the dream is an attempted wish-fulfilment. The reason why terror —a mixture of emotions of which one component is fear—is aroused, is that the object or situation represented in the dream has more than one meaning. Thus, while one tendency or wish is being satisfied, another tendency may be activated by the appreciation of a second symbolical meaning. When the emotion connected with the second meaning is fear, the way is open for the production of a terror dream; or, if the dream is developed further, of an anxiety dream. Then the dream fails to act as the "guardian of sleep."

The essay is a contribution to the literature of dreams.

The Kingdom of the Mind. By JUNE E. DOWNEY. New York: The Macmillan Co. 1927. Pp. xiv + 207. 8*s*. 6*d*. net.

This is the third volume of a series of books on scientific subjects written especially for "young people." The preparation of semi-popular books is always difficult, and the difficulty is perhaps enhanced when the subject matter is psychology, and the prospective readers are boys and girls; but Professor Downey has succeeded and has produced a readable little book which will appeal to many older "boys" and "girls" who take an interest in scientific affairs. From it they will learn a great deal about the functioning of the body and of the mind, and, what is more, how they can experiment on most of the problems for themselves.

Mental Hygiene. By DANIEL WOLFORD LA RUE, Ph.D. New York: The Macmillan Co. 1927. Pp. x + 443. 10*s*. net.

This work which, we are informed, is to carry to Mr La Rue's fellow-students "the working truth about mental hygiene as it appears" to Mr La Rue "after several years' study and teaching of mental hygiene" is divided into four parts: The first part deals with the forces that determine personality, and in it La Rue points out that mental health and disease can both in part be learned. The second part deals with the inheritance and development of traits, the third with mental hygiene of adult life, and the fourth with that of childhood and adolescence. Each chapter is followed by class exercises, problems for further study, topics for special investigation and report, and references to other works. Though a large amount of "phrenomental energy" has probably been expended on its production, the finished product is unsatisfactory. On p. 25 we are informed that a person is a "bio-mental unit." This is amplified and, on p. 28 we reach the conclusion that "Body causes mental changes, and mind causes bodily changes." Later, p. 143, we learn that "the hysteriac...is afflicted with a disease which does not really exist, but is brain-built only.... (p. 144) His weak nervous system is the victim of a stoppage or inhibition at some of its synapses." Presumably La Rue means that mind *may* cause bodily changes and *vice versa*, but why a disease which does not exist should be "brain-built" it is hard to conceive. Presumably, again, he means that there is no sufficient physical cause for the hysterical symptom, that the cause is mental; but a patient with a hysterical paralysis *is* as really diseased as another patient who is suffering from pneumonia. On p. 160 we are informed that "some of the more common disturbances of the neuroses which have also, or may have, a mental aspect, are apoplexy, commonly known as 'shock,' due to disturbance of cerebral circulation which may or may not be caused by syphilis...." Presumably, again, everything in this world, or out of it, "may or may not be caused by syphilis." On p. 360 we learn that "Fear, as an emotion, lies low in the brain." But enough has been said. It is a pity that so much study should have resulted in such loose, inaccurate, and often misleading exposition.

Intelligence Tests: Their Significance for School and Society. By W. F. DEARBORN, M.D., Ph.D. Published by the Houghton Mifflin Company. 1928. Pp. xxiv + 336.

In the course of nine lectures, which are printed in this volume, Professor Dearborn covers many aspects of the theory and practice of intelligence testing. He thinks there is no doubt that the intelligence tests which he discusses do give the teacher indications of the very capacity that he is trying to judge in a much more unsatisfactory way by school examinations. It is, then, a book of special value to the teacher rather than to the psychologist. The book is very interesting, but is possibly too elaborate in conception, while much of the discussion is somewhat redundant.

A Manual of Individual Mental Tests and Testing. By AUGUSTA F. BRONNER, WILLIAM HEALY, GLADYS M. LOWE, and MYRA E. SHIMBERG. Published by Little, Brown, and Company, Boston. 1927. Pp. x + 287.

The earlier chapters of this book are simply a list of mental tests, there being no discussion of the psychological significance or theoretical value of them. In the case of each test the details of methods of scoring, and a table of norms are given, except in the cases of those in which the standards have not been fully worked out. Later in the book some space is devoted to discussion of the claims made for the various tests previously described. It seems that many of the tests referred to might be used in the laboratory, and even for research, in connection with experiments involving introspection; they are, however, presented in this book merely as tests.

Love's Creation. By MARIE CARMICHAEL. London: John Bale, Sons and Danielsson, Ltd. 1928. Pp. 416. 7*s.* 6*d.* net.

This is a novel by Dr Marie Stopes. It makes interesting reading. In it her characters talk, probably as no people ever have talked, or ever will talk, about palaeontology, biology and philosophy. The hero, whose views the reader is clearly expected to approve, believes that a new synthesis of the sciences, based upon the essential unity of larger and larger groups of living elements, is approaching. He endeavours, with the utmost enthusiasm, to expound this belief.